SCIENCE:

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THE PHOTOPHONE.

Mr. Alexander Graham Bell, whose contributions to electric science have been recognized at home and abroad to their fullest value, has written a paper on his latest invention, the Photophone, which we reproduce and abundantly illustrate.

It is a beautiful application of the telephone to the registration of the mechanical action of electricity set in motion by light; but it is not (as the world was lead to suppose by some ill informed journalist) a method of transmuting light pulsations into electrical ones, and then changing these back again into light. A beam of light is reflected upon a mirror diaphragm, which is set in motion by the voice or otherwise; the concentrated ray is then reflected so as to affect a piece of selenium in a telephonic circuit, which, by its varying conductivity, acts intermittently on the diaphragm of the telephone, and thus in the usual way reproduces the sound. The instrument is simplicity itself, but the results are of the highest popular and scientific interest.

That it is possible for even the ray of a star to produce a mechanical effect, was demonstrated when Edison used his Tasimeter for measuring the waves of radiant energy of Vega. We thought Bell had solved the problem, upon which Edison was at work when he became interested in the perfection of his electric light, but our hope has not been realized. The subject, however, is one of extreme interest, and it is not strange for the discoverers of the two telephonic systems to be simultaneously engaged in solving the natural corollary to their great propositions. But Edison has an advantage in the pursuit. His employment of the varying electrical conductivity of carbon allows him to introduce any amount of reserve power for mechanical purposes.

It is much to be regretted that Edison can not find leisure from the practical applications of his science to turn his attention to those problems which he is so eminently capable of solving. We vividly recall some experiments in this direction which he told us of during the Spring of 1878, while on a visit to his laboratory at Menlo Park. He allowed a beam of light to fall on the surface of a diaphragm connected with his carbon button, in the hope that by a surface and molecular action, it would be possible to transmit its motion to a receiving diaphragm, where a similar molecular tension would result in the reproduction of the original vibrations. A faint halo is said occasionally to have surrounded the diaphragm. We could not but believe this due to the excited imagination of Mr. Edison, for at the time he was enthusiastically engaged in testing the wondrous capacity of the tasimeter, which he was soon to use in eclipse observations on the Draper expedition.

He also tried to observe the effect of a beam acting on the diaphragm of a phonograph, whose cylinder revolved at enormous speed, hoping a line of phosphorescence might arise from the tinfoil where it came in contact with the needle. Mr. Edison said he employed the direct action of the light (in the last case), in preference to using electricity as a medium for it, because he feared there existed a difference between the vibratory periods of light and electricity, although their velocity was nearly the same. For a similar reason he sought to realize the instantaneous translation of light by using his motograph, in preference to the magnetic telephone which for this purpose is valueless, owing to the time required to charge and discharge the iron core. But the most interesting of these experiments is to come. He threw a beam of lamp light on a small mirror, fastened to a tuning fork, and reflected a ray upon a strip of hard rubber in the tasimeter, the button of the latter being in circuit with a telephone and battery. On setting the fork in motion, the Lissajous figure caused a movement of the rod, which resulted in the reproduction of the musical note.

But all these pretty experiments are but introductory to the more subtile question, how to translate light through other forms of motion back into light. We wish a hearty rivalry between the two discoverers; for Messrs. Bell and Edison will find the fields of science (like those of trade) yield best fruit when fertilized by competition.

We have received a copy of the Report made by Professor S. W. Burnham, to the "James Lick Trust," of Observations made on Mt. Hamilton, with reference to the location of Lick Observatory, but we are compelled by press of matter to postpone further reference to it until a future date.

We have authority for stating that the Rev. W. H. Dallinger, of England, has consented to become Governor and Professor of Natural Sciences, of Wesley College, Sheffield. We congratulate the trustees of this establishment on having secured the assistance of one who has done so much to elevate the standard of scientific research.

The published papers of Professor Dallinger are models of their kind, and largely quoted by the highest authorities who write on the progress of Biology.

We trust Professor Dallinger, in taking the management of Wesley College, may still be enabled to prosecute his exhaustive microscopical studies, by the methods originally devised by himself, which have already been so fruitful of results, and promise to revolutionize our knowledge of such forms of life.

We are requested to state by the trustees of the Lick Observatories that they will be glad to receive the publications of Observatories, and of Astronomical and Scientific societies, for the permanent library of the Lick Observatory. They inform us that the preliminary work on Mt. Hamilton has already been commenced, and will be prosecuted as rapidly as possible under the circumstances. The small equatorial of 12-inch aperture, has been ordered of Alvan Clark & Sons, and will be placed in position early in 1881; and the great equatorial, meridian circle, and other instruments, will be contracted for at an early day. It is not expected there will be any further delay in putting the Lick Observatory in complete working order, other than that incident to the importance and magnitude of the undertaking.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.*

GENERAL BUSINESS—MONDAY, AUGUST 30TH.

The fifth day of the meeting was devoted to general business, to essays in the departments, and to visiting Salem in the afternoon. In the general session some new members were elected, and it was agreed that when the Association adjourned, that it should be to Cincinnati, on August 17, 1881.

The following reports were made:

Mr. E. B. Elliott, on an uniform system of registering deaths, births and marriages; Prof. E. L. Youmans, on the treating of science in public schools; Mr. F. B. Hough, on the preservation of forests; Prof. Harkness also reported certain amendments to the condition of the Association, to be acted on next year. At present there are two full sections in the association, and it is proposed to establish eight, covering the following branches; A, Physics; B, Astronomy and Mathematics; C, Chemistry and its Application; D, Mechanical Science; E, Geology and Geography; F, Biology; G, Anthropology; H, Economic Science and Statistics. A permanent sub-section of Microscopy is also provided for. These changes will bring the association in close resemblance to that of the British association.

The reading of the papers in the various sections was continued, the subjects of which need not here be stated, as we shall offer a full tabulated list of all the papers read before this association, conveniently arranged for future reference.

TUESDAY, AUGUST 31ST.

The list of essays entered for reading was closed with the number 280. The following officers were elected for the Cincinnati meeting to be held in 1881:—President, Professor G. J. Brush, of Yale College; Vice-President of Section A, Professor A. M. Mayer, of Hoboken; General Secretary, C V. Riley; Secretary, Section A, Professor John Trowbridge, of Harvard; Secretary, Section B, William Saunders; Treasurer, W. S. Vaux, of Philadelphia; Auditing Committee, Henry Wheatland, of Salem, and Thomas Meehan, of Philadelphia. Resolutions were adopted for a social re-union of the various sections on the second evening of future sessions. Resolutions were also passed recognizing the services to science of the late General Myer of the Signal Service, and the providing for the appointment of a committee to select a series of stars of stellar magnitude for standards, to be reported at the next meeting. Cable congratulations were sent to Michel Eugene Chevreul, senior member of the French Academy upon the completion of his ninety-fifth year. The reading of papers continued.

WEDNESDAY, SEPTEMBER 1ST.

The seventh and last day of the meeting was opened at the Institute of Technology, which had been found so convenient and well adapted for all purposes of the Association. Mr. George Engelman, of St. Louis, Mo., was chosen vice-president of the Natural History Section. The following gentlemen were elected a committee on stellar magnitudes: Professor E. C. Pickering, chairman, L. Boss, S. W. Burnham, Asaph Hall, William Harkness, E. S. Holden, Simon Newcomb, C. H. F. Peters, Ormond Stone and C. A. Young. The committee is to select a list of standard stars, to which the magnitudes of other stars may be referred. The following gentlemen were elected a committee on standard time: O. Stone, chairman, S. P. Langley, E. C. Pickering, J. R. Eastman, L. Boss, Leonard Waldie, J. K. Rees, G. W. Hough and H. S. Pritchett. The following resolution was passed:—

Dr. Charles T. Jackson, one of the founders and an early president of the Association of American Naturalists and Geologists, having, after many years of illness and seclusion, just passed away, it is fitting that this Association express its high appreciation of his long and valuable services, both as an original investigator in American geology and mineralogy, and as a teacher of chemistry, which will cause his name to be long held in honor and in grateful remembrance.

The following resolutions were passed on Tuesday:

Resolved, That the American Association for the Advancement of Science recognizes the value of contemporaneous observations at numerous and well-selected stations, and with standard instruments, as a first and indispensable condition of converting meteorology from a chaotic mass of useless facts into a science.

Resolved, That this Association acknowledges its obligations to the first secretary of the Smithsonian Institution for originating, supporting and cherishing such a system of meteorological observations throughout the vast domain of the United States until it had outgrown the resources of the institution, had justified its continuance by proved usefulness, and had awakened the fostering interest of the government.

Resolved, That, in the opinion of this Association, the welfare of commerce and agriculture, and the comfort of every member of the community have been promoted by the weather reports and weather charts which have been issued by the chief signal service at Washington, while they have, at the same time, furnished food for scientific thought.

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* Continuation of Report from SCIENCE, Sept. 4.

Resolved, That the Association feel and would hereby express the great loss which this service has suffered in the recent death of its chief officer, General A. J. Myer, whose energetic administration of novel duties, seconded by his able corps of scientific assistants, has commanded universal respect at home and abroad.

Professor N. P. Lupton, of Vanderbilt University, was added to the committee on the best methods of scientific teaching in the public schools. The following were chosen a committee on the registration of deaths, births and marriages: E. B. Elliott, F. B. Hough, J. B. Kellebrew, Joseph S. Copes and E. T. Cox.

It was voted yesterday to accept the invitation from Montreal for the meeting of the Association in 1882.

CONCLUSION.

The sections had all adjourned in the afternoon. In the evening a general session was held in Huntington Hall, President L. H. Morgan in the chair. About 250 ladies and gentlemen were present. A committee was appointed to confer with the President of the United States on the appointment of a chief signal officer. The committee includes Professors Brush and Barker, Dr. Bell, President Gilman, Professor Harkness, Mr. L. H. Morgan, Professor Clarke and Mr. A. Hall. The Association voted its thanks to those who had helped toward making the re-union of 1880 so pleasantly successful. The respective resolutions were supported by remarks from Professor Harkness, Judge Henderson, Professor Nason, the Rev. Mr. Shackelford, Professor Lattimore, Dr. J. Lawrence Smith, the Messrs. Hovey and Procter, and from the chair. The American Association for the Advancement of Science was then pronounced adjourned, to meet again, for the thirtieth time, at Cincinnati, on the 17th of August, 1881.

WE continue the publication of the addresses, and offer this week that by Dr. Asaph Hall, of Washington, and the Eulogy, by Professor A. M. Mayer, on the late Professor Joseph Henry, both of which we present in full; also abstracts from the following papers prepared by the authors:—The Photophone, by A. G. Bell; Mounds of Illinois, by William McAdams; Determination of the Comparative Dimensions of Ultimate Molecules, by W. N. Norton; Plan of the Cerebro-Spinal Nervous System, S. V. Clevenger; Observations of the Planetary Nebulæ, by E. C. Pickering; Co-efficients of Gas Solutions (Cut) by E. L. Nichols and A. W. Wheeler; The Wyandottes, by J. W. Powell; Ancient Agricultural Implements of Stone, by William McAdams; The Endo-Cranium and Maxillary Suspensorium of the Bee, by George Macloskie; Further Notes on the Pollination of Yucca, and on Pronuba and Prodoxus, by C. V. Riley; Simple Device for Projecting the Vibration of Liquid Films without a Lens, by H. S. Carhart; On Land Snails of the Palæozoic Period, by J. W. Dawson; The Structure of Mica Veins in North Carolina, by W. C. Kerr; Transformation of Planorbis, by A. Hyatt; The Languages of the Iroquois, by Mrs. E. A. Smith.

ADDRESS BY PROFESSOR ASAPH HALL.

Fellow-Members of the Association:—

Astronomy, in some of its forms, reaches back to the most distant historical epochs, and the changes that it has undergone during this long lapse of time give to this science a peculiar interest. In no other branch of human knowledge have we such a long and continuous history of the search after truth, of the painful struggle through which men have passed in freeing themselves from theories approved by the wise of their own times, and in overthrowing beliefs which had become incorporated into the life and culture of those times. Perhaps the grand array of the heavens, and the vast phenomena which they display, naturally led men to the invention of complicated theories; but these passed away at last before the test of observation, and the criticism of sceptical men; and the Copernican theory of our solar system, Kepler's laws of elliptical motion, and the Newtonian law of gravitation, gave to Astronomy a real scientific character.

The discovery of the laws that govern the motions of the heavenly bodies, and the construction of the theory of these motions, demanded from practical Astronomy better observations and a more accurate determination of the orbits of the planets and the moon, or of the constants that enter into the problems of celestial mechanics; and this demand led to an improvement in the instruments, and in the art of observing. The astronomers and instrument-makers of England and France led the way in these improvements. The great national observatories of those countries were established, and in England Flamsteed and Sharp, Bird and Bradley, were foremost in raising practical Astronomy to the condition of satisfying the demands of theory. But theoretical Astronomy was soon to receive a wonderful advancement. Perhaps no one contributed more powerfully to this progress than Lagrange. The writings of this man were models of simplicity and elegance, and yet so complete and general are his investigations that they contain the fundamental theorems of celestial mechanics. By the invention and perfection of the method of the variation of the arbitrary constants of a problem, and by the establishment of the differential equations of a planetary orbit depending on the partial differential coefficients of a single function, Lagrange reduced the question of perturbations to its simplest form, and gave the means of deducing easily the most interesting conclusions on the past and future condition of our solar system. To supplement this great theorist there was needed another kind of genius. Combining the highest mathematical skill with unequalled sagacity and common sense in its application, Laplace gathered up and presented in a complete and practical form the whole theory of celestial mechanics. Besides his numerous and brilliant discoveries in theoretical Astronomy, Laplace gave us some of the finest chapters ever written on the theory of attraction,* and a complete treatise on the calculus of probability.

By such labors as these the questions of Astronomy were brought into order and classified, and the attention of Astronomers was directed better than ever before to the determination of the quantities which must be found from observation. Moreover, the refinement of analysis and the completion of theory brought out new and more delicate questions, not less interesting, and requiring more complete investigation and more powerful instruments. The careful examination and study of the instruments and methods of observation became necessary, as well as complete and rigorous methods of reduction; and finally there was needed a critical and satisfactory method for the discussion of observations. For these last improvements in Astronomy we are indebted chiefly to the astronomers and mechanics of Germany.

Among those who contributed by means of their optical and mechanical skill to furnish Astronomy with the instruments necessary for its further advancement, no one holds a more honorable place than Joseph Frauenhofer. This man began his scientific work at the age of twenty-two, and died at thirty-nine, and yet in those seventeen years he gave to Astronomy great improvements in the manufacture of optical glass, driving clocks for equatorials, and telescopes and micrometers, that in the hands of Bessel and Struve gave to observations a degree of accuracy hardly thought of before. To such men as Frauenhofer and his co-workers, who have carried on and improved the construction of instruments of precision, practical Astronomy owes much; and yet, after all, the principal thing in a science is the man himself. No matter how excellent the instruments may be, the question whether they shall be used for the advancement of the science, and shall contribute the full value of their peculiarities to help towards increasing the accuracy of astronomical determinations.

* "Ein schönes Document der feinsten analytischen Kunst."—GAUSS

depends wholly on the astronomer. Again, astronomy is now so completely a science, and all its operations are so closely connected with theory, that no one is fit to have charge of an extended series of astronomical observations who has not a fair amount of theoretical knowledge. Without such knowledge his labor is apt to be thrown away, and is never so effective.

As a good example of what the modern astronomer should aim to be, we may take Bessel. To this man we owe a large part of our best methods for the examination and determination of the errors of our instruments, and the introduction of complete and rigorous methods for the reduction of observations. Bessel's reduction and discussion of Bradley's observations was a masterpiece of its kind, bringing out the value of Bradley's work, which had lain unnoticed for more than half a century, and forming a starting-point for sidereal astronomy. This work was continued and perfected in his tables for the reduction of astronomical observations, published twelve years afterwards; a work that has done more than anything else to introduce order and system into practical astronomy. In the discussion of instruments and the determination of their error, Bessel's conception of an instrument was that of a geometrical figure, and the positions of the lines and divisions of this instrument were considered with corresponding rigor. Although devoted almost entirely to astronomy, yet Bessel was an able mathematician, and of this he has left abundant proof. It seem to be necessary that a man should die and be forgotten personally before his work can be fairly estimated; but time adjusts these matters at last, and I know of no astronomer whose work promises to endure the judgment of the future better than that of F. W. Bessel.

It has been said that for producing the most puzzling compound of metaphysics and mathematics, something which has neither height nor depth, nor length nor breadth, and which no one can understand, the German mathematician is unequalled. And at the same time it must be said that, for clearness of conception, and beauty and precision of expression, Germany has produced in Gauss a mathematician who is unsurpassed, and who is worthy of a place by the side of Lagrange. Omitting all reference to the works of Gauss in theoretical astronomy and in geodesy, which are many and important, I refer here only to his method for the discussion of observations, and of deducing the most probable values of our constants. Almost the entire work of astronomy is a vast system of numerical approximation, in which the first steps are obvious and easy, but where the theory soon becomes complicated and the labor enormous. Thus the calculation of the approximate orbit of a planet or of a comet is the work of only a few hours; but the computation of the perturbations, and the correction of the elements from all the observations, may be the work of months and years. It is therefore of the highest importance that we should have a method for the discussion of observations that will give us the best result, and which will introduce order and system into this department of astronomy. Such a method is that of least squares. For the complete theory of this method, and for nearly all the arrangements and algorithms necessary for its practical application, we are indebted to Gauss. The invention and application of this method to the discussion of observations of all kinds seems to me one of the greatest improvements of modern times, and its proper use will lead to a steady progress in astronomy. We must remember, however, that this method does not undertake the improvement of the observations themselves, as some have seemed to think; but, when rightly used, it produces simply the best result we can hope for from a given series of observations. It does not, therefore, dispense with skill and judgment on the part of the astronomer, but one is tempted to say that, if he has not these prime qualities, then the next best thing for him to have is the method of least squares. The use of this method has become one of the chief characteristics of modern astronomy, and if we compare the results of its application with those of the older methods, we shall see its superiority. Thus, for example, no astronomer of to-day, who is accustomed to the modern methods of discussion, would be satisfied with the manner in which Bouvard represents in his tables the observations of Jupiter and Saturn, but would suspect at once some error in his theory of the motions of these planets.

The present condition of astronomy is the result of the continued labors of our predecessors for many generations; and to this result the lapse of time itself has largely contributed. For the full development of the secular changes of our solar system, for an accurate knowledge of the proper motions of the stars of our sidereal universe, and of the great changes of light and heat that are going on among them, the astronomer must wait until future ages. It is his present duty to prepare for that future by making the observations and investigations of his own day in the best manner possible; and to do this needs a careful consideration of the present condition of the science. Although the objects for observation have become so numerous, and the range of investigation so wide, that there is room for the most varied talent and skill, yet there is danger that there may be a waste of labor, either in duplicating work, or in doing it in an improper manner. Especially may this happen in observations of the principal planets of our system, and of the fixed stars. In the case of the planets the observations are abundant, and the orbits are already well determined, except that of Neptune, for which, on account of its slow motion, we must of necessity wait for time to develop its small peculiarities, if such there be. For all these planets the observations at one or two observatories are amply sufficient, and even then the observations ought to be confined to a short time near the opposition, or at quadrature, and so made that they may be easily combined into a single normal position, which will suffice for the theoretical astronomer. To scatter such observations over a period of several months is to throw away one's labor, and to leave to the computer the disagreeable duty of rejecting a part of the observations as useless. It seems to me, therefore, unwise for several observatories to continue heaping up observations of the four outer planets of our system, when ten observations a year of each planet will give all the data that are needed. Again, for all the principal planets, observation is now in advance of theory, except, perhaps, in the case of one or two of them. Thus, for Saturn, all the tables are decidedly in error, and, although an attempt has been made to accuse the observations of this planet, it is quite certain that the trouble lies in the theory; for in the case of Jupiter and Saturn we have the most complicated planetary theory of our system, and one that has not yet been completely developed. It seems to me, also, that observations of our moon might well be confined to one or two observatories. Here again observation is far in advance of theory, if indeed there be now in use a pure lunar theory. All the lunar ephemerides that we have are affected with empirical terms, and the lunar theory itself remains an unsolved mystery. In this case there is no attempt to impeach the observations. The trouble seems to be with the perturbations of long period, and this does not call for numerous observations during each lunation. By a proper consideration of these matters astronomers may, I think, save themselves much useless labor.

Observations of the fixed stars are of the utmost importance in astronomy, since the positions of the stars are the fundamental points on which depends our knowledge of the motions of the planets, the moon, and of the stars themselves; and it is on account of this fact that Bessel's tables, published in 1830, were of such great service, since they introduced correct and elegant methods of reduction, and clearly defined all the constants and epochs. We now have the positions of several hundred stars so well known that they may be safely used in the reduction of observations; and for these accurate positions we are largely indebted to the astronomers of the Pulkowa Observatory, who have made such absolute determinations a special work. There is still an opportunity for the improvement of these positions, and every well-executed determination will be of value; but it is doubtful if crude and irregular observations can add anything to our knowledge of the positions of these stars. Neither can the routine, mechanical style of observing, that is apt to prevail in large observatories, be of much use here. It would be better in most

cases for such observatories to assume the positions of the fundamental stars, and to leave the further improvement of their places to skillful astronomers who understand the theory of such work, and who carefully study and become masters of their instruments. In these refined observations the refraction of light by our atmosphere also plays an important part, and this question will need to be examined at every observatory that undertakes to do independent work. It is true that every new and good meridian instrument may, and perhaps ought, to contribute something towards removing constant errors, and giving us a more accurate knowledge of a star's position; but when this position is very well known, the only way for further improvement is through complete and careful observations, and their thorough reduction and discussion.

In the observations of double stars but little had been done before the present century, and the labors of W. Struve form the real starting-point in this branch of astronomy. These labors have been ably continued by his son, the present director of the Pulkowa Observatory, and the observations of these two astronomers, extending over a period of nearly sixty years, are of the greatest value for our knowledge of the motions of the double stars. This is a branch of the science into which irregular workers are apt to enter, and where some of them have done good service; but if any amateur astronomer will compare his own work with that of the Struves, and will study the methods followed by them in determining their personal and instrumental errors, and will emulate the steadiness with which they have followed out their purpose, he can do much to enhance the value of his labor. Here the observations are simple, and easily reduced, and the chief requisites are skill and patience on the part of the observer. He should not be discouraged because he obtains no immediate or great reward for his work, or public notice, or because some one who rants about the nebular hypothesis and kindred subjects, of which he knows nothing, is for a time the great astronomer of the day. The observer will learn finally that a good observation of the smallest double star, or of the faintest comet or asteroid, is worth more than all such vague talk. The observation has a positive value, however small, but the physical theories of the universe, of which modern popular science is so productive, are generally worse than useless.

The first step towards a rational and trustworthy knowledge of our sidereal universe must come from a determination of the distances of the stars. The solution of this problem was attempted soon after the Copernican theory of our solar system was established, when it was seen that we have a long base line for our measures, or the diameter of the earth's orbit, and it was supposed that the solution would be easy. These early trials were all failures, but they led to some very interesting and important discoveries, such as Bradley's discovery of the aberration of light; to the knowledge of the fact that the determination of the parallaxes, or the distances of the stars, although simple in theory, is practically a difficult question; and then to an improvement in the instrumental means of observation, to a careful study of the methods of observation and the instruments, and to a recognition of the necessity of a complete and rigorous reduction of the observations. An examination of these early attempts is an instructive study. It is only about forty years ago that the solution of this problem was at last attained, and then only by the application of the most powerful instruments and the best observing skill. An interesting result of the determinations of stellar parallax is obtained at once in the check it puts on speculations concerning the structure of the sidereal universe. The first astronomers who considered the parallaxes of the stars very naturally assumed that the bright stars are nearer to us than the faint ones, and therefore they observed the bright stars for parallax. Now, while this assumption may be true as a general statement, the actual determinations of parallax show that some of the faint stars which are not visible to the naked eye are much nearer to us than the brightest stars of our northern sky. Again it was assumed that a large proper motion is a certain index of a star's nearness to us; but observation shows that this also may be an erroneous assumption. This is a problem whose solution is only just begun, but already we know enough of its difficulties to see that we need the most powerful micrometrical apparatus that can be brought into use. The invention of some micrometer that, while as accurate as the present filar micrometer, would give the observer a much greater range of observation, and enable him to select suitable stars of comparison, is something much to be desired. At present the heliometer seems to be the best instrument for observations of this kind. Formerly it was thought that photography would furnish a good method for such delicate determinations; but so far the photographic methods have not given the necessary degree of accuracy in the measurements, and the astronomical use of photography is confined mostly to descriptive astronomy, where, especially in solar eclipses, it has rendered excellent service. Closely connected with the parallaxes of the stars and their proper motions is the interesting question of determining their motions to or from our sun, according to the theory of Doppler. Here likewise the numerical determinations are so discordant, that we cannot have much confidence in the results. In both these cases we need more powerful apparatus, and a complete and thorough investigation of the methods of observation. Perhaps some of the large instruments now constructing may be employed in these methods, and we may soon have better results.

A great advance has been made in catalogueing the fainter stars. This work was begun by the French astronomers nearly a century ago, and was continued by Bessel, Argelander, and others. An important step towards the completion of this work was taken by Argelander and his assistants in their great catalogue of the approximate positions of 324,198 stars, which was finished in 1861. This census of the stars will soon be extended, we hope, over the whole heavens; and it already forms the groundwork for the great zone observations of stars now going on in Europe and in this country, and which must be nearly finished. These observations will doubtless reveal many interesting cases of the proper motion of the stars, and will certainly form the basis for a knowledge of the motion of our solar system in space, and for sidereal astronomy generally, such as we have never had before. Our American observatories can render a good service by observing stars of southern declination, since our observatories are ten or twelve degrees farther south than those of Europe, and thus have an advantage of position which ought to be made use of; and which may serve to unite into a harmonious system the observations made in the northern and southern hemispheres. The work of mapping the very faint stars near the ecliptic has also been greatly extended, and it is to this extension that we owe the rapid increase in the number of the small planets between Mars and Jupiter. But besides aiding in the discovery of the asteroids, accurate charts of the small stars have a permanent value in giving us a knowledge of the heavens at their epoch, and also some idea of the distribution of the stars in space.

It is an interesting question whether, among the thousands of nebulæ that are scattered over the heavens, any of them show changes of form or of brightness. These objects seem to be at least as distant as the stars, and as they have sometimes an area of several degrees, they must be bodies of an enormous extent. That changes are going on in these bodies seems probable, but to be visible at such distances the changes must be very great. In this case there is need of much caution in the discussion of the drawings made at different epochs, and by different astronomers with telescopes of different power; since the nebulæ change their appearance with the telescope used, with different conditions of the air, and with a variation of their altitude above the horizon. Here the excellent photometers that have been recently invented, and which are being so well applied to the determination of the brightness of the stars, may give us assistance. Perhaps also new drawings of the nebulæ, and their criticism and discussion, and a full recognition of the difficulties of making such drawings, will soon lead to a decision of the question of their change of form. Since the study of the light of the stars with new and improved photometers has now become a specialty, we may look for more exact and continued observations of the variable stars. This is a matter of which we know but little, and it is one where a persevering observer may do

good service. Although he may not find any immediate encouragement in the discovery of remarkable relations among these stars, or the probable cause of their variability, he will be collecting observations that must form the test of every theory. As examples of the result of intelligent and persevering observation, we have the case of the sun spots, which led directly to the discovery of their period and its singular variability; and that of the shooting stars, which has shown us a very curious relation between these meteors and the comets, and one which may open to us the most extensive views of the relations between our own solar system and other systems in space.

The present condition of astronomy, with its vast and rapidly increasing store of accurate observations, offers many interesting subjects to the theoretical astronomer. The observations of the stars are now so numerous, and have been so fully reduced and criticised, and the time during which the observations have been made is so extended, that we shall soon have excellent data for a new and very exact determination of the constant of precession. The orbits of the planets and the moon, and their masses, are now so well known that little uncertainty can arise from this source; and by taking into the calculation a great number of stars in different parts of the heavens, we may be able to determine the motion of the solar system in space, as well as the constant in precession. The constant of aberration also needs a new determination; and since this constant is so closely connected with the theory of light and its velocity, and the methods of its determination are still under discussion, it would be well if several astronomers could determine this constant independently. The value we now use was found by W. Struve from prime-vertical observations, and is apparently very accurate; but no astronomical constant should depend on the work of a single astronomer with a single instrument, when it can be determined so easily and by other methods. The old method of finding the value of this constant from the eclipses of Jupiter's satellites may yet give us a trustworthy value. The value of the other constant necessary for the reduction of observations, that of nutation, must be nearly that found by Peters in his well-known investigation of this question. This value may be verified by a new series of observations of Polaris, or of the declinations of stars situated so that this constant has its full influence on the reductions.

There are many subjects in astronomy that need investigation, but in most cases the labor required is very great, and the completion of the work would occupy a long time. This follows of course from the fact that, with the refinement of observations and their exact reduction, many small terms must be considered which formerly could be neglected. The lunar theory has been a vexed question for the last two centuries, and may remain so for a long time to come. This will no doubt be the case until some able astronomer, with the will and perseverance of Delaunay, shall undertake its complete revision. This question should now be looked on as a purely scientific one, and its definite solution should be undertaken. The theory should not be patched up by guesswork to fit the observations, but should be carried out with the utmost rigor. This is a problem to which a young and able mathematician may well devote his life, and we must expect its solution from some such clear-headed devotee of science. Several of the planetary theories need a new investigation, and some of them are already in the hands of able astronomers. That of Mercury is especially interesting in connection with the intra-Mercurial planets, and it is to be hoped that Leverrier's theory of this planet may soon have a careful revision.

Again, among the secondary systems, the satellites of Jupiter and Saturn offer many interesting questions to the astronomer. At present the satellites of Jupiter demand a more complete theory, and new tables of their motions. Corrected elements of these satellites may be required for reducing observations of their eclipses, and for deriving a new value of the constant of aberration. These satellites form a peculiar and interesting system, and their theory is so complicated that the labor of correcting their elements and forming new tables would be great, but still within the power of a persevering astronomer. The recent discovery of the connection of comets with streams of meteors has given additional interest to cometary astronomy, and there is plenty of hard work to be done in reducing observations, in computing perturbations, and in deducing the best orbits of the comets. The periodical comets have another interest, since they may give us information concerning the matter filling space. It seems to be probable from different reasons, such as the consideration of the light of the stars, that there must be matter spread throughout the celestial spaces; but the only heavenly body that has directly given us information on this subject is Encke's comet, which has a period of $3\frac{1}{3}$ years. For a long time the motion of this comet was very completely computed by Encke, whose calculations show very strong proof of a resisting medium. These calculations were continued by Von Asten, whose early death prevented him from finishing his work, and the theory of this comet is left in an unsatisfactory condition. It is very desirable that the motion of this comet should be completely investigated, and although the method of the special perturbations of the elements followed by Encke is probably the best that can be used, still in such a case it would be well to apply various methods. Here again, on account of the frequent returns of the comet, the labor of computation is very great, and probably would be enough fully to occupy the time of one astronomer. The interesting questions connected with the motion of this comet ought to induce some one to undertake this laborious work, and these questions are so important that two or three astronomers might well be employed on its theory.

The methods of astronomy have now become so well established, that the future advancement of the science is assured, especially since long intervals of time give an increased value to observations. Yet we may hope for improvement in instruments, for the introduction of new methods of observing, for better trained and more efficient astronomers; and perhaps also the rapid advancement of the physical sciences may furnish us with new and more powerful methods of investigation. There is an intimate relation between the instrument-maker and the astronomer, and they should understand each other better than is generally the case. It may seem a small matter that the divisions of a circle, or of a scale, should not be too finely or too coarsely cut; that the reading scale should not be placed in an inconvenient position, and that the illumination of the instrument should be carefully studied, and brought under the control of the astronomer; but these are really essential points, and, if not rightly arranged, are certain to weary the observer and to impair the quality of his work. Such mistakes will not be remedied until the makers better understand the uses of an astronomical instrument, and have correct ideas of the ends to be attained. Since our American opticians have placed themselves at the head of their craft, we may hope that our instrument-makers will do likewise, and that they will soon be able to furnish us with the best instruments of precision.

There is one point to which astronomers should give more attention, and from which we may reasonably hope that great advantages to astronomy may come; and that is to the selection of sites for new observatories. It is possible, perhaps probable, that our instruments may be greatly enlarged and improved, and that important discoveries and improvements in the manufacture of optical glass may be made; but it seems certain that we have within easy reach very decided advantages for astronomical work by the choice of better positions for our instruments. Very few American observatories have been established for the purpose of doing scientific work, or with much thought or care for their condition; but generally they are built in connection with some college or academy, and are the product of local and temporary enthusiasm, which builds an observatory, equips it with instruments, and then leaves it helpless. The atmosphere that surrounds us, and its sudden changes of temperature, are the great obstacles to the good performance of a telescope; and the larger the instrument, and the higher the magnifying power, the more serious are these hindrances. Now, with our present means of travel, we can easily place our instruments at an altitude of eight or ten thousand feet, and above a large part of the atmosphere. In this way we may be able to do

with small instruments what at common altitudes can be done only with large ones; and when possible it is always better to use small instruments, since they are more easily handled, and are relatively stronger and better than large ones. Uniformity of temperature may be secured by seeking locations in the tropical islands, or on the coasts like that of California, where the ocean winds keep the temperature nearly uniform throughout the year. At great altitudes we may secure a clearness of vision that would be of the greatest value in the examination of faint objects, and by this means, and by persevering and continuous observation, interesting discoveries may be made. It is a matter of course that, except in the case of comets, the future discoveries in astronomy will belong to faint and delicate objects; but these are interesting, and should not be neglected. A uniform temperature, which secures good definition, and steady images of the stars, is necessary for accurate determinations of position, and for all measurements of precision. This condition is especially important in such work as that of stellar parallax, the determination of the constant of aberration, and wherever the yearly change of temperature may act injuriously. In the selection of better sites for observatories, I think we have an easy means of advancing astronomy.

As this science grows and expands, it will become more and more necessary to study the economy of its work, in order that astronomers may bestow their labors in the most advantageous methods, and may rid themselves of all cumbersome and time-consuming processes. The manner of publishing observations has already been much abbreviated, and improved, I think, by some of the European astronomers, and this change seems destined to become universal. As the positions of many objects are now well known, the need of printing all the details of the observation, such as the transit of the wires, the readings of the micrometers, etc., is very slight; and this printing may be safely abandoned. Even this change will lead to a great saving in the time and cost of printing. But this will necessitate a more complete discussion of the work and a more careful examination of the instruments; things to be desired, since they tend to lift the observer out of his routine, and make him a master of his business. There are objections to this change, and some of them are real, such as the importance of publishing a complete record; but this is overestimated, I think, since the original records ought always to be referred to in case of doubt; and other objections are factitious, such as the need of publishing a large a showy book in order to impose on the public.

We may hope also for improvements in theoretical astronomy, and for the better training and preparation of students of this science. I know that it is sometimes said that theoretical astronomy is finished, and that nothing more can be done. Such assertions come from professors who are old and weary, or from those young men who tire out early in life; but they are wrong. The improvements that Hansen has made in the theory of perturbations, and Poinsot's study of the theory of rotation, show what careful investigation may do, and assure us of further progress. It must be confessed that some of the astronomical work done in our country bears evidence that the astronomers did not understand the correct methods of reduction, and much of it shows evidence of hasty and ill-considered plans. This is perhaps a natural condition for beginners, but we trust that it has been outgrown. An actual need for the astronomical students of our country is a good book on theoretical astronomy, similar to Pontécoulant's work, in which the whole subject shall be presented in a complete form, such as we find in the *Mécanique Céleste*, together with an account of the improvements made by Gauss, Poisson, Hansen and others. There is no American book of this kind, and the English works are too partial, designed apparently to fit the student for college examinations, and not to give him a complete knowledge of the science. Such a book has hardly been attempted in our language, unless that of Woodhouse may be an exception, and it may be a long time in coming, since it requires a man qualified to do the work, and will involve an expense of labor in the preparation, and of cost in publishing, such as few are willing to incur. In the meantime it is far better for the student to go directly to the writings of Lagrange and Laplace, of Gauss and Poisson and other masters, rather than to spend time in reading second-rate authors who endeavor to explain them. And generally this will be found the easier way also, since the student avoids the confused notions and symbols, and the grotesque expressions and egotism of small men, and is lifted into the region of ideas and invention.

In presenting his exposition of the nebular hypothesis, which has since become so celebrated, Laplace says: "I present this hypothesis with the distrust which everything ought to inspire that is not a result of observation or of calculation." It is a singular fact that, among all the writings on the nebular hypothesis, I have never seen a reference to this presentation of it by its most distinguished advocate; and yet this is the true spirit of scientific astronomy. Laplace did not wish to exempt his own theories from criticism, and neither should anyone. In astronomy there is no final human authority, no synod or council, but simply an appeal to reason and observation. If a theory or a discovery be true, it will stand the test of observation and of calculation; if false, it must pass away to that Miltonian limbo where so many things have gone and are going. The question is sometimes asked, of what use is astronomy? and the reply generally made is that it has conferred great benefits on navigation and on commerce, since it is by means of his astronomical knowledge that the sailor determines the position of his ship on the ocean. There is a truth in this reply, but it is only partial. The great value of astronomy is that it is really a science, and that it has broken the path and led the way through which all branches of science must past if they ever become scientific. It is the spirit of honest, unrelenting criticism, and of impartial examination, that finally eliminates error and awards to every one his just due, that makes astronomy honorable and attractive; and it is by cultivating this spirit that astronomy confers its chief benefit, for it is this that shall break in pieces and destroy all false assumptions in science and in philosophy.

JOSEPH HENRY.

EULOGY BY PROFESSOR A. M. MAYER.

At the meeting of the Association in 1878, a committee, composed of Professors Baird, Newcomb and myself, was appointed to prepare a eulogy on our revered and lamented colleague and former president, Joseph Henry. This—I will not say labor, but duty of affection—has devolved on me alone. I would that the other members of this committee had laid before you their tributes to his memory, because for years they had been closely associated with him in his social and professional life in Washington. Yet, while Professor Henry had been the friend of their manhood, he was the friend of my boyhood; and during 25 years he ever regarded me—as was his wont to say—with a "paternal interest." To his disinterested kindness and wise counsels is due much, very much, of whatever usefulness there is in me. Hence I have said that it is a duty of affection for me to speak to you about one who was my beloved friend. I shall not, however, attempt a biography of Joseph Henry, nor will I speak of his administrative life as director of the Smithsonian Institution, for this is known and valued by the whole world. His best eulogy is an account of his discoveries; for a man of science, *as such*, lives in what he has *done*, and not in what he has *said*; nor will he be remembered in what he proposed to do. I will, therefore, with your permission, confine myself chiefly to Henry *as the* discoverer; and I do this the more willingly because I am familiar with his researches, and also because Professor Henry, from time to time, took pleasure in giving me accounts of these mental conceptions which preceded his work, led him to it and guided him in it. Rightly to appreciate a discoverer, we should not look at his work from our time, but go back and regard it from his time; we should not judge his work in the fulness of the light of present knowledge, but in the dim twilight which alone illuminated him to then unknown—but now well-known—facts and laws. I will, therefore, endeavor first to present you with a clear, but necessarily very concise, view of the state of our knowledge of electricity when Henry began his original researches in that branch of science, and then point out the value of his discoveries, by showing that they added to knowledge, and how they instigated and influenced the discoveries and inventions of other men. Henry began his electrical researches at the age of twenty-eight, in the year 1827, while he was professor of mathematics and natural philosophy in the Albany Academy. At these he continuously worked till 1832, when, at

the age of thirty-three, he moved to Princeton College. After a year's break in his work, caused by the preparation of his course of lectures for the college, he is again at original research, and continues his contributions to electrical discoveries till 1842. Thus, during fourteen years, between the ages of twenty-eight and forty-three, he was a constant and fertile worker.

As with many other men of originality, Henry's first essays were in the direction of improving the means of illustrating well-established scientific facts and principles. His first paper of October, 1827, is interesting because it was his first. In it he improves on the usual apparatus which had been used by Ampère and others to show electro-dynamic actions, by employing several turns of insulated wire, instead of one, as had previously been the practice. Thus, for example, to show the directive action of the earth's magnetism on a freely-moving closed circuit, Henry covered copper wire with silk, and then made out of it a ring about twenty inches in diameter, formed of several turns of the wire. The extremities of this wire were soldered to zinc and copper plates. The coil was then suspended by silk filaments. On plunging the metal plates into a glass of dilute acid the ring rotated around its point of suspension till its plane took a permanent position at right angles to the magnetic meridian. By a similar arrangement of two concentric coils, one suspended within the other, he neatly showed the mutual actions of voltaic currents flowing in the same or opposite directions, which facts are the foundations of Ampère's celebrated law. We now reach a period when Henry appears as a discoverer, and truly one of no mean order. As I remember his narration to me in the year 1859, it was as follows: He said that one evening he was sitting in his study in Albany with a friend, when, after a few moments of reverie, he arose and exclaimed, "Tomorrow I shall make a capital experiment!" For several months he had been brooding over Ampère's electro-dynamic theory of magnetism, and he was then deeply interested in the phenomena of the development of magnetism in soft iron, as shown in the experiments of Arago and Sturgeon. At the moment he had arisen from his chair it had occurred to him that the requirements of the theory of Ampère were not fulfilled in the electro-magnets of Arago and of Sturgeon, but that he could get those conditions which the theory required by covering the developing wire with a non-conductor, like silk, and then wrapping it closely around the soft iron bar in several layers; for the successive layers of wire coiling first in one direction and then in the other would tend to produce a resultant action of the current at right angles to the axis of the bar; and furthermore, the great number of convolutions thus obtained would act on a greater number of molecules of the bar, and therefore exalt its magnetism. "When this conception," said Henry, "came into my brain, I was so pleased with it that I could not help rising to my feet and giving it my hearty approbation." Henry did go to work next day, and to his great delight and encouragement discoveries of the highest interest and importance revealed themselves to him week after week. When he had finished his newly conceived magnet he found that it supported several times more weight than did Sturgeon's magnet of equal size and weight. This was his first original discovery.

I will now give, as far as possible, Henry's own words in narrating the subsequent investigations of these very interesting phenomena: "The maximum effect, however, with this arrangement and a single battery was not yet obtained. After a certain length of wire had been coiled upon the iron the power diminished with a further increase of the number of turns. This was due to the increased resistance which the larger wire offered to the conduction of electricity. Two methods of improvement, therefore, suggested themselves. The first consisted, not in increasing the length of coil, but in using a number of separate coils on the same piece of iron. By this arrangement the resistance to the conduction of the electricity was diminished and a greater quantity made to circulate around the iron from the same battery. The second method of producing a similar result consisted in increasing the number of elements of the battery, or, in other words, the projectile force of the electricity, which enabled it to pass through an increased number of turns of wire, and thus, by increasing the length of the wire, to develop the maximum power of the iron. To test these principles on a larger scale an experimental magnet was constructed. In this a number of compound helices were placed on the same bar, their ends left projecting, and so numbered that they could be all united into one long helix, or variously combined in sets of lesser length. From a series of experiments with this and other magnets it was proved that, in order to produce the greatest amount of magnetism from a battery of a single cup, a number of helices is required; but when a compound battery is used, then one long wire must be employed, making many turns around the iron, the length of wire and consequently the number of turns being commensurate with the projectile power of the battery. In describing the results of my experiments the terms *intensity* and *quantity* magnets were introduced to avoid circumlocution, and were intended to be used merely in a technical sense. By the intensity magnet I designated a piece of soft iron so surrounded with wire that its magnetic power could be called into operation by an intensity battery; and by a quantity magnet a piece of iron so surrounded by a number of separate coils that its magnetism could be fully developed by a quantity battery. "I was," said Henry, "the first to point out this connection of the two kinds of the battery with the two forms of the magnet, in my paper in Silliman's Journal, January, 1831, and clearly to state that when magnetism was to be developed by means of a compound battery one large coil was to be employed, and when the maximum effect was to be produced by a single battery a number of strands were to be used."

We will now return to Henry's study of the properties of his intensity magnet. This magnet was formed of a piece of iron one-fourth of an inch in diameter, bent in the U form and wound with eight feet of insulated wire. His batteries were two,—one formed of a single element with a zinc plate four inches by seven, surrounded by copper and immersed in dilute acid; the other, a Cruikshank's battery, or trough, with twenty-five double plates. The plates of this battery were joined in series, and altogether had exactly the same surface of zinc as that in the single-cell battery. The magnet was now connected directly to the single cell. The magnet held up seventy-two ounces. Then five hundred and thirty feet of number 18 copper wire led the current from the cell to the magnet; it now supported only two ounces. Five hundred and thirty feet more of the wire were introduced into the circuit, and then the magnet held but one ounce. In these facts Henry faced the same results as confronted Barlow five years before, and caused Barlow then to say: "In a very early stage of electro-magnetic experiments it had been suggested [by Laplace, Ampère and others] that an instantaneous telegraph might be established by means of conducting wires and compasses, but I found such a sensible diminution with only two hundred feet of wire, as at once to convince me of the impracticability of the scheme"; and such, at that day, seemed to be the common opinion of men of science. But this opinion is presently to be shown by Henry to be ill-founded, by reason of the ignorance of the relations which have of necessity to exist between the kind of battery and the kind of magnet in order to produce electro-magnetic action at a distance—relations which Henry was the first to discover. This accomplishment justly entitles him to be regarded as a man of genius and a discoverer of no mean order. This discovery will always remain the one important fact that was to be known, to be understood, and to be applied, before it was possible to have constructed any form of electro-magnetic telegraph. Let us see how Henry made this discovery.

After ending the experiments with the one-cell battery and reaching results which seemed to confirm the opinion of Barlow as to the "impracticability of the scheme" of an electro-magnetic telegraph, Henry attached his magnet to the second battery formed of twenty-five cells, arranged in series. The current from this battery was sent to the magnet through 1060 feet of the same wire as had been used in the experiments with the first battery of one cell. The magnet now lifted eight ounces. It had held up only one ounce, when with the same length of interposed wire the battery of one cell was used. He now attached his electro-magnet directly to the poles of the 25-cell battery, when, to his astonishment, it only held seven ounces. The same magnet it will be remembered, when attached to the one-cell battery, supported seventy-two ounces. Here were facts of the highest significance, and Henry was not slow to seize them in all their bearings. Referring to these experiments, he said in 1857: "These steps in the advance of electro-magnetism, though small, were such as to interest and astonish the scientific world. These developments were considered at the time of much importance in a scientific point of view, and they subsequently furnished the means by which magneto-electricity, the phenomena of dia-magnetism, and the magnetic effects in polarized light were discovered. They gave rise to the various forms of electro-magnetic machines which have exercised the ingenuity of inventors in every part of the world, and were of immediate applicability in the introduction of the magnet to telegraphic purposes. Neither the electro-magnet of Sturgeon nor any electro-magnet ever made previous to my investigations was applicable to transmitting power to a distance."

Not satisfied with the mere statement that his discovery was "directly applicable to Mr. Barlow's project of forming an electro-magnetic telegraph," he actually constructed one, some time during the year 1831, around one of the upper rooms of the Albany Academy. It was more than a mile in length, and made signals by sounding a bell. This was the first electro-magnetic telegraph which had worked through so great a length of wire. It was the first "sounding" electro-magnetic telegraph. The relative parts played by Henry and Morse are described in Henry's "Statement" published by the Smithsonian in 1857. "*The principles,*" says Henry, "*I had developed were applied by Dr. Gale to render Morse's machine effective at a distance.*" This statement seems to be as direct, as clear, as truthful, and as comprehensive as one can desire. I will take the liberty of remarking that had Henry taken out a patent in which he claimed as his invention an electro-magnet formed of two or more layers of insulated wire, Morse's patent would not have been so valuable. Remember, I speak not of the merit of the invention, but of the merit of the patent; for the invention, so far as Morse is concerned, would have remained the same, because one essential part of a Morse telegraph is Henry's intensity magnet, and certainly Morse never invented that.

If Ohm's law had been known to Henry, with all of its consequences, when applied to his discovery of the exaltation of the electro-magnetism of iron, in connection with his discovery of the

proper relations necessary between batteries and magnet to get the greatest electro-magnetic effects, his discoveries would appear dwarfed, though yet of excellent workmanship. But did he at this time, 1827 to 1832, know of Ohm's law? I infer that Henry arrived at his discoveries independently of such knowledge, and for two-fold reasons. First, Ohm's law was published as late as 1827, in Berlin, and was received almost contemptuously. Henry was unable to read German, and Ohm's papers were first published in English in 1841. Secondly, from the manner in which Henry worked at his problems and viewed his results, I conclude that he had no knowledge of Ohm's laws; else why should he have been astonished at the effects when his intensity magnet was connected with his intensity battery? Henry, now in possession of powerful magnets, began to work on another problem. He tried to do the reverse of what he had already done. His magnet was made by the action of the electric current, and he now tried to obtain an electric current from the magnet; and he succeeded. Henry and Faraday independently discovered the means of producing an electric current and spark from a magnet. Tyndall speaks of this experimental results as the "Mont Blanc of Faraday's own achievements." A few words now will place Henry in his proper and just relations to these important discoveries. All the information he had received about Faraday's discovery was the account of Faraday's production of magneto-electricity by the sudden insertion of a magnet into a helix and its sudden withdrawal therefrom. Henry's experiment is entirely different, and certainly was entirely original with him; but it is essentially identical with another of Faraday's of which Henry had no knowledge. Thus it appears that, although Henry cannot be placed on record as the *first* discoverer of the magneto-electric current, he stands alone as its *second* independent discoverer.

Henry's next discovery was that of the induction of a current upon itself, or of the extra current, as it is sometimes called. Here he anticipated Faraday by nearly two years and a half in the observation of the fundamental facts. Notwithstanding an explicit disclaimer of Faraday, the credit of this discovery has been generally given to the latter. This is accounted for by the fact that, although Henry anticipated others in his observations, he had not leisure to follow them up to their full explanation until after Faraday had completely unraveled their nature. In 1838, after his return from a first visit to Europe, Henry discovered an entirely new class of phenomena in electrical induction. He first showed that an induced current may excite a second induced current in a neighboring closed conductor, that this last may induce a third current, and so on. These currents Henry styled currents of the first, second, third, etc., orders, and he showed that they alternate in their direction successively. He investigates the difference in these currents as they flow through different resistances. The same phenomena he tracks through the inductive sections of the discharge of the Leyden jar and of the frictional electrical machine, and shows how they differ from those produced by the voltaic battery. These researches are the most finished of Henry's investigations, and will ever be regarded as models of careful and thorough scientific work.

Henry had a versatile mind, and did not confine his attention to the study of electricity. His researches in molecular physics, though not extensive, are remarkable. Here his suggestions and methods have stimulated others to follow in the paths which he has pointed out. In 1839 Henry made a curious discovery as to the permeability of lead to mercury. He found mercury would even ascend a lead wire to the height of a yard in a few days. He even made what might be called syphons of lead, which would nearly empty a vessel of mercury by drawing the fluid over its sides. Subsequently, in 1845, with Mr. Cornelius, he proved that copper, when heated to the melting point of silver, would absorb the latter metal. In 1844 Henry was investigating the nature of the forces acting in liquid films. Studying the tenacity of the soap-bubble film, although his experiments could only furnish approximate results, they showed that the molecular attraction of water for water is really several hundred pounds to the square inch, and probably equal to the attraction of ice for ice. Another of Henry's investigations, having a practical bearing, should be more widely known than it is. Among his duties as chairman of the United States lighthouse board was the testing of the various physical properties of the oils submitted to the government for purchase. Fluidity was one of these properties for which it seemed most difficult to get reliable tests. Here he very ingeniously applied the theorem of Torricelli, which shows that equal quantities of all liquids of equal fluidity will flow out of an orifice in equal times. Henry found that with different oils the flow of equal quantities differed, the rapidity of flow of sperm oil exceeding that of lard oil in the ratio of 100 to 167. Alcohol proved to be less fluid than water. Henry took a deep interest in acoustics. His additions to this science were chiefly the results of experiments upon fog signals. He made extensive experiments with various sound-producing instruments, and eventually decided in favor of the steam syren fog-horn. He determined that these instruments send their sound farthest when tuned very near to the treble C, and he also showed the uselessness of applying reflectors to them. During eleven years Henry sought to advance the efficiency of our fog signals by experiments in all weathers. Many very puzzling facts were collected. Thus it was observed that a sound coming to a mariner against the wind would cease to be audible on the deck of his vessel while it continued to be heard at the masthead. It was also observed that upon approaching a fog-horn from a distance the intensity of sound would gradually increase, then die down rapidly, become inaudible through a space of three or four miles, and perhaps not reappear until the vessel was within a mile of the instrument. These facts demanded explanations, and for a long time remained enigmas to Henry, till one day he met with a paper by Professor Stokes, in which the effect of an upper current in deflecting a wave of sound is fully explained. This hypothesis of Stokes Henry was able to apply to the solution of the problems in question.

Henry's services to the light-house board were of great value to the country. The fact that his investigations showed that lard oil heated to about 250° Fahrenheit is superior in fluidity and illuminating power to sperm oil caused the substitution of the former for the latter. A dollar a gallon was saved, which amounts to about one hundred thousand dollars a year in favor of the government. In light and heat Henry made several investigations which we must pass over. One, however, is so important that it cannot be omitted. I refer to his application of the thermopile in determining the distribution of heat on the optical images of distant objects. In a bold, and wonderful experiment, he sought to study the distribution of heat on the surface of the sun. In 1845, with Stephen Alexander, he formed an image of the sun, by means of a telescope, upon a screen. In this screen was cut an aperture, closed by the surface of a thermopile. By a motion of the telescope, any part of the image could be brought upon the pile. A solar spot being present, he clearly proved that it emitted less heat than the surrounding parts of the luminous disc. This method of research was shown to Secchi. On his return to Europe the latter made no small repute by extending these observations, using Henry's methods, but often, I fear, not giving full credit to the originator. But let that pass, for the bread which Henry cast upon the waters has returned to our own shores, thanks to the genius of our colleague Langley.

It is impossible to crowd into one brief hour the thoughts which were his occupation during more than half a century. I have at least endeavored to exhibit the more important part of the labors of his life. What shall we think of them? Surely they are on as high a plane as those of any of his contemporaries, and show as much originality as theirs in their conception—as much skill in their execution. Yet it has been said that Henry was not a man of genius. As I have not been able to find that the philosophers who have the special charge of giving from time to time definitions of genius, have been able to come to any satisfactory conclusion among themselves, I will leave their company, and, with your liberty, take my definition from a book which, if we accredit Thackeray, is one of the very best, if not the best, novel ever written in English. After listening to this I will allow you to form your own opinions as to whether Henry did or did not possess genius. "By genius I would understand that power, or rather those powers, of the mind which are capable of penetrating into all things within our reach and knowledge, and of distinguishing their essential differences. These are no other than invention and judgment, and they are both called by the collective name of genius, as they are of those gifts of nature which we bring with us into the world. Concerning each of which many seem to have fallen into very great errors; for by invention, I believe, is generally understood a creative faculty, which would indeed prove most romance writers to have the highest pretensions to it; whereas by invention is meant no more, and the word so signifies, than discovery in finding out; or, to explain it at large, a quick and sagacious penetration into the true essence of all the objects of our contemplation. This, I think, can rarely exist without the concomitancy of judgment, for how we can be said to have discovered the true essence of two things, without discovering their difference, seems to me hard to conceive. Now this last is the undisputed province of judgment; and yet some few men of wit have agreed with all the dull fellows in the world in representing these two to have seldom or never been the property of one and the same person." My own judgment, if of any value, would rank the ability of Henry—I do not say his achievements—a little below that of Faraday. Indeed their lives and their manners of working were strangely alike. Faraday was the son of a blacksmith. He once wrote: "I love a smith's shop and anything relating to smithery. My father was a smith." Henry's father plied a schooner on the Hudson. Each started in life with moral and benevolent habits, well-developed and healthy bodies, quick and accurate perceptions, calm judgment and self reliance, tempered with morality and good manners—a good ground, surely, in which to plant the germs of the scientific life. Faraday was an apprentice to a bookbinder. Henry served in the same capacity under a blacksmith. Each, endowed with a lively imagination, was in his younger days fond of romance and the drama; and, by a singular similarity of accidents, each had his attention turned to science by a book which chance threw in his way. This work in the case of Faraday was "Mrs. Marcet's Conversations on Chemistry," and the book which influenced Henry's career was "Gregory's Lectures on Experimental Philosophy, Astronomy and Chemistry." Of Mrs. Marcet's book Faraday thus writes:—"My Dear Friend,—Your subject interested me deeply every way; for Mrs. Marcet was a good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at

the age of thirteen, in the year 1804, remaining there eight years, and during the chief part of the time bound books. Now it was in those books, in the hours of the week, that I found the beginning of my philosophy. There were two that especially helped me,—the Encyclopædia Britannica, from which I gained my first notions of electricity, and Mrs. Marcet's "Conversations on Chemistry," which gave me my foundation in that science. Do not suppose that I was a very deep thinker, or was marked a precocious person. I was a burly imaginative person, and could believe in the Arabian Nights as easily as in the Encyclopœdia. But facts were important to me and saved me. I could trust a fact and always cross-examined an assertion. So when I questioned Mrs. Marcet's book by such little experiments as I could find means to perform, and found it true to the facts as I could understand them, I felt that I had got hold of an anchor in chemical knowledge, and clung fast to it. Thence my deep veneration for Mrs. Marcet—first, as one who had conferred great personal good pleasure on me; and then as one able to convey the truth and principle of those boundless fields of knowledge which concern natural things to the young, untaught and inquiring mind. You may imagine my delight when I came to know Mrs. Marcet personally; how often have I cast my thoughts backward, delighting to connect the past and the present; how often, when sending her a papers as a thank-offering, I thought of my first instructress; and such thoughts will remain with me."

Henry wrote on the inside of the cover of Gregory's work the following words: "This book, although by no means a profound work, has, under Providence, exerted a remarkable influence on my life. It accidentally fell into my hands when I was about sixteen years old, and was the first book I ever read with attention. It opened to me a new world of thought and enjoyment; invested things before almost unnoticed with the highest interest; fixed my mind on the study of nature, and caused me to resolve at the time of reading it that I would immediately begin to devote my life to the acquisition of knowledge.—J. H." Each of these philosophers worked with simple instruments, mostly constructed by his own hands, and by methods so direct that he appeared to have an almost intuitive perception into the workings of nature; and each gave great care to the composition of his writings, sending his discoveries into the world clothed in simple and elegant English. Finally each loved science more than money, and his Creator more than either. There was sympathy between these men; and Henry loved to dwell on the hours that he and Bach spent in Faraday's society. I shall never forget Henry's account of his visit to King's College, London, where Faraday, Wheatstone, Daniell and he had met to try and evolve the electric spark from the thermopile. Each in turn attempted and failed. Then came Henry's turn. He succeeded, calling in the aid of his discovery of the effect of a long interpolar wire wrapped around a piece of soft iron. Faraday became as wild as a boy, and, jumping up, shouted, "Hurrah for the Yankee experiment." And Faraday and Wheatstone reciprocated the high estimation in which Henry held them. During a visit to England, not long before Wheatstone's death, he told me that Faraday and he had, after Henry's classical investigation of the induced currents of different orders, written a joint letter to the council of the Royal Society, urging that the Copley medal, "that laurel wreath of science," should be bestowed on Henry. On further consultation with members of the council it was decided to defer the honor till it would come with greater *éclat*, when Henry had continued farther his researches in electricity. Henry's removal to Washington interrupted these investigations. Wheatstone promised to give me this letter to convey to Henry as an evidence of the high appreciation which Faraday and he had for Henry's genius, but Wheatstone's untimely death prevented this. Both Faraday and he gave much thought to the philosophy of education, and in the main their ideas agreed. I may in this connection be excused from reading abstracts from a letter from Henry soon after he had received the news I had given my son his name. After a playful discusssion of the name Joseph, Jo and Josey, he says—what may be news to most of you: "I did not object to Henry as a first name; although I have been sorry that my grandfather, in coming from Scotland to this country, substituted it for Hendrie, a much less common, and, therefore, distinctive name." He then proceeds: "I hope that both his body and mind will be developed by proper training and instruction, that he may become an efficient, wise and good man. I say efficient and wise, because these two characteristics are not always united in the same person. Indeed, most of the inefficiency of the world is due to their separation. Wisdom may know what ought to be done, but it requires the aid of efficiency to accomplish the desired object. I hope that in the education of your son due attention may not only be given to the proper development of both these faculties, but also they will be cultivated in the order of nature; that is, doing before thinking; art before science. By inverting this order much injury is frequently done to a child, especially in the case of the only son of a widowed mother, in which a precocious boy becomes an insignificant man. On examination in such a case it will generally be found that the boy has never been drilled into expertness in the art of language, of arithmetic, or of spelling, of attention, perseverance and order; or, in other words, of the habits of an active and efficient life."

Henry was a man of extensive reading, and often surprised his friends by the extent and accuracy of his information, and by the original manner in which he brought his knowledge before them. Not only was he well versed in those subjects in which one might naturally suppose him proficient, but in departments of knowledge entirely distinct from that in which he gained his reputation as an original thinker. Although without a musical ear he had a nice feeling for the movement of a poem, and was fond of drawing from his retentive memory poetic quotations apt to the occasion. He was a diligent student of mental philosophy, and also took a lively interest in the progress of biological science, especially in following the recent generalization of Darwin; while the astonishing development of modern research in tracking the history of prehistoric man had for him a peculiar fascination. Yet with all his learning, reputation and influence, Henry was as modest as he was pure. One day, on opening Henry's copy of Young's Lectures on Natural Philosophy—a book which he has studied more than any other work of science—I read on the fly-leaf, written in his own hand, these words:—

> "In Nature's infinite book of secrecy
> A little I can read."—*Shakespeare.*

And did he not read a little "in Nature's infinite book of secrecy?" And did he not read that little well? May we all read our little in that book as modestly and as reverently as did Joseph Henry.

THE PHOTOPHONE.

By Alexander Graham Bell.

In bringing before you some discoveries made by Mr. Sumner Tainter and myself, which have resulted in the construction of apparatus for the production and reproduction of sound by means of light, it is necessary to explain the state of knowledge which formed the starting point of our experiments. I shall first describe the remarkable substance selenium, and the manipulations devised by various experiments: but the final result of our researches has evidenced the class of substances sensitive to light-vibrations, until we can propound the fact of such sensitiveness being a general property of all matter. We have found this property in gold, silver, platinum, iron, steel, brass, copper, zinc, lead, antimony, German silver, Jenkin's metal, Babbitt's metal, ivory, celluloid, gutta percha, hard rubber, soft vulcanized rubber, paper, parchment, wood, mica and silvered glass; and the only substances from which we have not obtained results are carbon and thin microscopic glass. We find that when a vibratory beam of light falls upon these substances they emit sounds,—the pitch of which depends upon the frequency of the vibratory change in the light. We find farther that, when we control the form or character of the light-vibration on selenium, and probably on the other substances, we control the quality of the sound and obtain all varieties of articulate speech. We can thus, without a conducting wire as in electric telephony, speak from station to station, wherever we can project a beam of light. We have not had opportunity of testing the limit to which this photophonic influence can be extended, but we have spoken to and from points 213 meters apart; and there seems no reason to doubt that the results will be obtained at whatever distance a beam of light can be flashed from one observatory to another. The necessary privacy of our experiments hitherto has alone prevented any attempts at determining the extreme distance at which this new method of vocal communication will be available. I shall now speak of selenium.

In the year 1817 Berzelius and Gottlieb Gahn made an examination of the method of preparing sulphuric acid in use at Gripsholm. During the course of this examination they observed in the acid a sediment of a partly reddish, partly clear brown color, which, under the action of the blow-pipe gave out a peculiar odor, like that attributed by Klaproth to tellurium. As tellurium was a substance of extreme rarity, Berzelius attempted its production from this deposit; but he was unable, after many experiments, to obtain further indications of its presence. He found plentiful signs of sulphur mixed with mercury, copper, zinc, iron, arsenic and lead, but no trace of tellurium. It was not in the nature of Berzelius to be disheartened by this result. In science every failure advances the boundary of knowledge as well as every success, and Berzelius felt that, if the characteristic odor that had been observed did not proceed from tellurium, it might possibly indicate the presence of some substance then unknown to the chemist. Urged on by this hope he returned with renewed ardor to his work. He collected a great quantity of the material, and submitted the whole mass to various chemical processes. He succeeded in separating successively the sulphur, the mercury, the copper, the tin and the other known substances whose presence had been indicated by his tests:—and after all these had been eliminated, there still remained

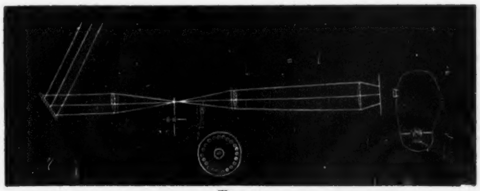


FIG. 1.

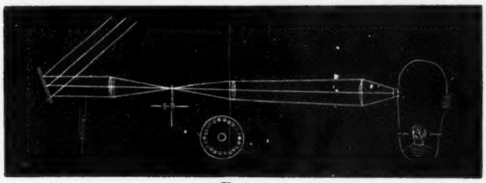


FIG. 2.

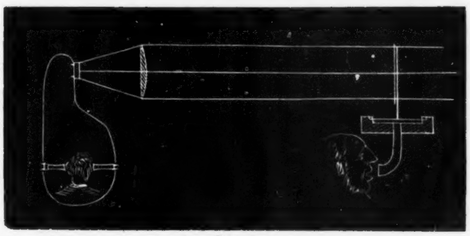


FIG. 3.

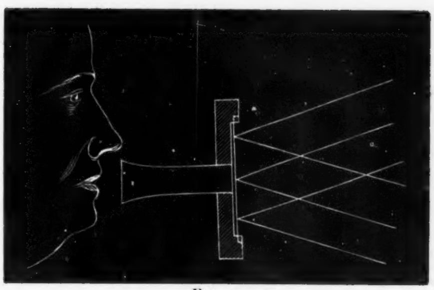


FIG. 4.

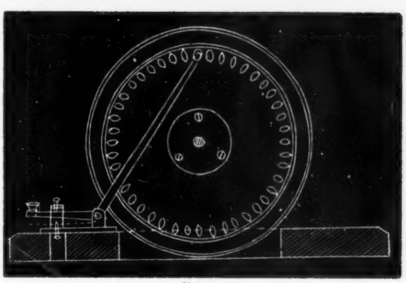


FIG. 5.

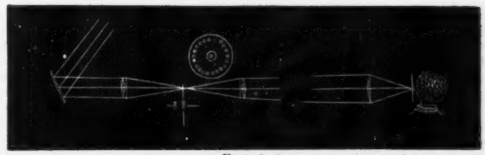


FIG. 6.

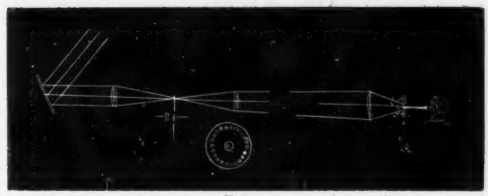


FIG. 7.

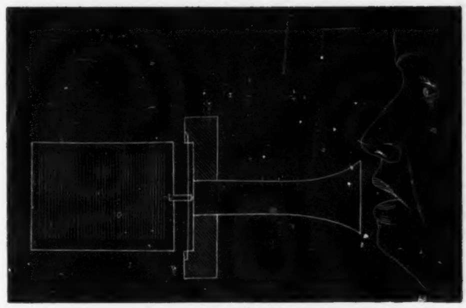


FIG. 8.

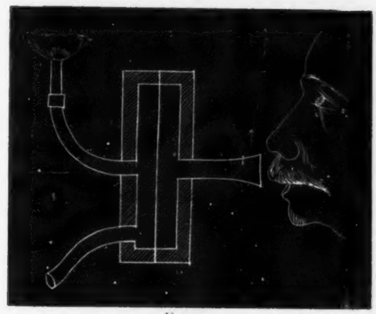


FIG. 9.

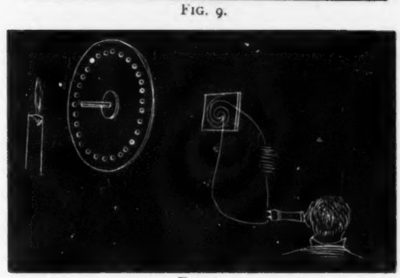


FIG. 10.

ILLUSTRATING PAPER ON PHOTOPHONE.

By ALEXANDER GRAHAM BELL.

1—The Interposition of Hard Rubber Plate. 2—The Light passed through openings in rapidly revolving diaphagm and reflected in selenious recesses. 3—Application of figure four. 4—Action of Voice on thin Plate of Silvered Mica. 5—Application of Morse system of Telegraphy to Photophone. 6—Listening directly to Receiving Plate. 7—Another form of Receiver. 8—One of the first forms, Voice passed through Slits. 9—Direct Action of Voice on Gas Flame. Action of Candle Light on selenius received.

a residue which proved upon examination to be what he had been in search of—a new elementary substance. The chemical properties of this new element were found to resemble those of tellurium in so remarkable a degree that Berzelius gave to the substance the name of "Selenium," from the Greek word *selene*, the moon—(" tellurium," as is well known being derived from *tellus*, the earth,

Although tellurium and selenium are alike in many respects, they differ in their electrical properties; tellurium being a good conductor of electricity, and selenium, as Berzelius showed, a non-conductor. Knox discovered, in 1837, that selenium became a conductor when fused; and Hittorff, in 1852, showed that it conducted, at ordinary temperatures, when in one of its allotropic forms. When selenium is rapidly cooled from a fused condition, it is a non-conductor. In this, its vitreous form, it is of a dark-brown color, almost black by reflected light, having an exceedingly brilliant surface. In thin films it is transparent, and appears of a beautiful ruby red by transmitted light. When selenium is cooled from a fused condition with extreme slowness, it presents an entirely different appearance, being a dull lead color, and having throughout a granulated or crystalline structure, and looking like a metal. In this form it is perfectly opaque to light, even in very thin films. This variety of selenium has long been known as "granular" or "crystalline" selenium, or, as Regnault called it, "metallic" selenium. It was selenium of this kind that Hittorff found to be a conductor of electricity at ordinary temperatures. He also found that its resistance to the passage of an electrical current diminished continuously by heating up to the point of fusion, and that the resistance suddenly increased in passing from the solid to the liquid condition. It was early discovered that exposure to sunlight hastens the change of selenium from one allotropic form to another; and this observation is significant in the light of recent discoveries.

Although selenium has been known for the last sixty years it has not yet been utilized to any extent in the arts, and it is still considered simply as a chemical curiosity. It is usually supplied in the form of cylindrical bars. These bars are sometimes found to be in the metallic condition; but more usually they are in the vitreous or non-conducting form. It occurred to Willoughby Smith that, on account of the high resistance of crystalline selenium, it might be usefully employed at the shore-end of a submarine cable, in his system of testing and signalling during the process of submersion. Upon experiment, the selenium was found to have all the resistance required—some of the bars employed measuring as much as 1400 megohms—a resistance equivalent to that which would be offered by a telegraph wire long enough to reach from the earth to the sun! But the resistance was found to be extremely variable. Experiments were made to ascertain the cause of this variability. Mr. May, Mr. Willoughby Smith's assistant, discovered that the resistance was less when the selenium was exposed to light than when it was in the dark.

In order to be certain that temperature had nothing to do with the effect, selenium was placed in a vessel of water, so that the light had to pass through from one to two inches of water in order to reach the selenium. The approach of a lighted candle was found to be sufficient to cause a marked deflection of the needle of the galvanometer connected with the selenium, and the lighting of a piece of magnesium wire caused the selenium to measure less than half the resistance it did the moment before.

These results were naturally at first received by scientific men with some incredulity, but they were verified by Sale, Draper, Moss and others. When selenium is exposed to the action of the solar spectrum, the maximum effect is produced, according to Sale, just outside the red end of the spectrum, in a point nearly co-incident with the maximum of the heat rays; but, according to Adams, the maximum effect is produced in the greenish-yellow or most luminous part of the spectrum. Lord Rosse exposed selenium to the action of non-luminous radiations from hot bodies, but could produce no effect; whereas a thermopile under similar circumstances gave abundant indications of a current. He also cut off the heat-rays from luminous bodies by the interposition of liquid solutions, such as alum, between the selenium and the source of light, without affecting the power of the light to reduce the resistance of the selenium; whereas the interposition of these same substances almost completely neutralize the effect upon the thermopile. Adams found that selenium was sensitive to the cold light of the moon, and Werner Siemens discovered that, in certain extremely sensitive varieties of selenium, heat and light produced opposite effects. In Siemens's experiments, special arrangements were made for the purpose of reducing the resistance of the selenium employed. Two fine platinum wires were coiled together in the shape of a double flat spiral in the zig-zag shape, and were laid upon a plate of mica so that the discs did not touch one another. A drop of melted selenium was then placed upon the platinum-wire arrangement, and a second sheet of mica was pressed upon the selenium, so as to cause it to spread out and fill the spaces between the wires. Each cell was about the size of a silver dime. The selenium cells were then placed in a paraffine bath, and exposed for some hours to a temperature of 210° C., after which they were allowed to cool with extreme slowness. The results obtained with these cells were very extraordinary; in some cases the resistance of the cells, when exposed to light, was only one-fifteenth of their resistance in the dark.

Without dwelling farther upon the researches of others, I may say that the chief information concerning the effect of light upon the conductivity of selenium will be found under the names of Willoughby Smith, Lieutenant Sale, Draper and Moss, Professor W. G. Adams, Lord Rosse, Day, Sabini, Dr. Werner Siemens and Dr. C. W. Siemens. All observations by these various authors had been made by means of galvanometers; but it occurred to me that the telephone, from its extreme sensitiveness to electrical influences, might be substituted with advantage. Upon consideration of the subject, however, I saw that the experiments could not be conducted in the ordinary way for the following reason: The law of audibility of the telephone is precisely analogous to the law of electric induction. No effect is produced during the passage of a continuous and steady current. It is only at the moment of change from a stronger to a weaker state, or vice versa, that any audible effect is produced, and the amount of effect is exactly proportional to the amount of variation in the current. It was, therefore, evident that the telephone could only respond to the effect produced in selenium at the moment of change from light to darkness, or vice versa; and that it would be advisable to intermit the light with great rapidity, so as to produce a succession of changes in the conductivity of the selenium, corresponding in frequency to musical vibrations within the limits of the sense of hearing. For I had often noticed that currents of electricity, so feeble as to produce scarcely any audible effects from a telephone when the circuit was simply opened or closed, caused very perceptible musical sounds when the circuit was rapidly interrupted, and that the higher the pitch of sound the more audible was the effect. I was much struck by the idea of producing sound by the action of light in this way. Upon farther consideration it appeared to me that all the audible effects obtained from varieties of electricity could also be produced by variations of light acting upon selenium. I saw that the effect could be produced at the extreme distance at which selenium would respond to the action of a luminous body, but that this distance could be indefinitely increased by the use of a parallel beam of light, so that we could telephone from one place to another without the necessity of a conducting wire between the transmitter and receiver. It was evidently necessary, in order to reduce this idea to practice, to devise an apparatus to be operated by the voice of a speaker, by which variations could be produced in a parallel beam of light, corresponding to the variations in the air produced by the voice.

I proposed to pass light through a large number of small orifices, which might be of any convenient shape, but were preferably in the form of slits. Two similarly perforated plates were to be employed. One was to be fixed and the other attached to the centre of a diaphragm actuated by the voice, so that the vibration of the diaphragm would cause the movable plate to slide to and fro over the surface of the fixed plate, thus alternately enlarging and contracting the free orifices for the passage of light. In this way the voice of a speaker could control the amount of light passed through the perforated plates without completely obstructing its passage. This apparatus was to be placed in the path of a parallel beam of light, and the undulatory beam emerging from the apparatus could be received at some distant place upon a lens, or other apparatus, by means of which it could be condensed upon a sensitive piece of selenium placed in a local circuit with a telephone and galvanic battery. The variations in the light produced by the voice of the speaker should cause corresponding variations in the electrical resistance of the selenium employed: and the telephone in circuit with it should reproduce audibly the tones and articulations of the speaker's voice. I obtained some selenium for the purpose of producing the apparatus shown; but found that its resistance was almost infinitely greater than that of any telephone that had been constructed, and I was unable to obtain any audible effects by the action of light. I believed, however, that the obstacle could be overcome by devising mechanical arrangements for reducing the resistance of the selenium, and by constructing special telephones for the purpose. I felt so much confidence in this that, in a lecture delivered before the Royal Institute of Great Britain, upon the 17th of May, 1878, I announced the possibility of hearing a shadow by interrupting the action of light upon selenium. A few days afterwards my ideas upon this subject received a fresh impetus by the announcement made by Mr. Willoughby Smith before the Society of Telegraph Engineers that he had heard the action of a ray of light falling upon a bar of crystalline selenium, by listening to a telephone in circuit with it.

It is not unlikely that the publicity given to the speaking telephone during the last few years may have suggested to many minds in different parts of the world somewhat similiar ideas to my own.

Although the idea of producing and reproducing sound by the action of light, as described above, was an entirely original and independent conception of my own, I recognize the fact that the knowledge necessary for its conception has been disseminated throughout the civilized world, and that the idea may therefore have occurred to many other minds. *The fundamental idea, on which rests the possibility of producing speech by the action of light, is the conception of what may be termed an undulatory beam of light in contradistinction to a merely intermittent one.* By an undulatory beam of light, I mean a beam that shines continuously upon the selenium receiver, but the intensity of which upon that receiver is subject to rapid changes, corresponding to the changes in the vibratory movement of a particle of air during the transmission o

a sound of definite quality through the atmosphere. The curve that would graphically represent the changes of light would be similar in shape to that representing the movement of the air. I do not know whether this conception had been clearly realized by "J. F. W.," of Kew, or by Mr. Sargent, of Philadelphia; but to Mr. David Brown, of London, is undoubtedly due the honor of having distinctly and independently formulated the conception, and of having devised apparatus—though of a crude nature—for carrying it into execution. It is greatly due to the genius and perseverance of my friend, Mr. Sumner Tainter, of Watertown, Mass., that the problem of producing and reproducing sound by the agency of light has at last been successfully solved.

The first point to which we devoted our attention was the reduction of the resistance of crystalline selenium within manageable limits. The resistance of selenium cells employed by former experimenters was measured in millions of ohms, and we do not know of any record of a selenium cell measuring less than 250,000 ohms in the dark. *We have succeeded in producing sensitive selenium cells measuring only 300 ohms in the dark, and 155 ohms in the light.* All former experimenters seemed to have used platinum for the conducting part of their selenium cells, excepting Werner Siemens, who found that iron and copper might be employed. We have also discovered that brass, although chemically acted upon by selenium, forms an excellent and convenient material; indeed, we are inclined to believe that the chemical action between the brass and selenium has contributed to the low resistance of our cells by forming an intimate bond of union between the selenium and brass. We have observed that melted selenium behaves to the other substances as water to a greasy surface, and we are inclined to think that when selenium is used in connection with metals not chemically acted upon by it, the points of contact between selenium and the metal offer a considerable amount of resistance to the passage of a galvanic current. By using brass we have been enabled to construct a large number of selenium cells of different forms. The mode of applying the selenium is as follows: The cell is heated, and, when hot enough, a stick of selenium is rubbed over the surface. In order to acquire conductivity and sensitiveness, the selenium must next undergo a process of annealing.

We simply heat the selenium over a gas stove and observe its appearance. When the selenium attains a certain temperature, the beautiful reflecting surface becomes dimmed. A cloudiness gradually extends over it, somewhat like the film of moisture produced by breathing upon a mirror. This appearance gradually increases, and the whole surface is soon seen to be in the metallic, granular or crystalline condition. The cell may then be taken off the stove, and cooled in any suitable way. When the heating process is carried too far, the crystalline selenium is seen to melt. Our best results have been obtained by heating the selenium until it crystallizes, and continuing the heating until signs of melting appear, when the gas is immediately put out. The portions that had melted instantly re-crystallize, and the selenium is found upon cooling to be a conductor, and to be sensitive to light. The whole operation occupies only a few minutes. This method has not only the advantage of being expeditious, but it proves that many of the accepted theories on this subject are fallacious. Our new method shows that fusion is unnecessary, that conductivity and sensitiveness can be produced without long heating and slow cooling; and that crystalization takes place during the heating process. We have found that on removing the source of heat immediately on the appearance of the cloudiness, distinct and separate crystals can be observed under the microscope, which appear like leaden snow-flakes on a ground of ruby red. Upon removing the heat, when crystalization is further advanced, we perceive under the microscope masses of these crystals arranged like basaltic columns standing detached from one another, and at a still higher point of heating the distinct columns are no longer traceable, but the whole mass resembles metallic pudding-stone, with here and there a separate snow-flake, like a fossil, on the surface. Selenium crystals formed during slow cooling after fusion present an entirely different appearance, showing distinct facets.

We have devised about fifty forms of apparatus for varying a beam of light in the manner required, but only a few typical varieties need be shown. The source of light may be controlled, or a steady beam may be modified at any point in its path. The beam may be controlled in many ways. For instance, it may be polarized, and then affected by electrical or magnetic influences in the manner discovered by Faraday and Dr. Ker. The beam of polarized light, instead of being passed through a liquid, may be reflected from the polished pole of an electro-magnet. Another method of affecting a beam of light is to pass it through a lens of variable focus. I observe that a lens of this kind has been invented in France by Dr. Cusco, and is fully described in a recent paper in "La Nature;" but Mr. Tainter and I have used such a lens in our experiments for months past. The best and simplest form of apparatus for producing the effect remains to be described. This consists of a plain mirror of flexible material—such as silvered mica or microscopic glass. Against the back of this mirror the speaker's voice is directed. The light reflected from this mirror is thus thrown into vibration corresponding to those of the diaphragm itself.

In arranging the apparatus for the purpose of reproducing sound at a distance, any powerful source of light may be used, but we have experimented chiefly with sunlight. For this purpose a large beam is concentrated by means of a lens upon the diaphragm mirror, and, after reflection, is again rendered parallel by means of another lens. The beam is received at a distant station upon a parabolic reflector, in the focus of which is placed a sensitive selenium cell, connected in a local circuit with a battery and telephone. A large number of trials of this apparatus have been made with the transmitting and receiving instruments so far apart that sounds could not be heard directly through the air. In illustration, I shall describe one of the most recent of these experiments. Mr. Tainter operated the transmitting instrument, which was placed on the top of the Franklin schoolhouse in Washington, and the sensitive receiver was arranged in one of the windows of my laboratory, 1325 L street, at a distance of 213 metres. Upon placing the telephone to my ear I heard distinctly from the illuminated receiver the words: "Mr. Bell, if you hear what I say, come to the window and wave your hat." In laboratory experiments the transmitting and receiving instruments are necessarily within earshot of one another, and we have therefore been accustomed to pooling the electric circuit connected with the selenium receiver, so as to place the telephones in another room. By such experiments we have found that articulate speech can be reproduced by the oxy-hydrogen light, and even by the light of a kerosene lamp. The loudest effects obtained from light are produced by rapidly interrupting the beam by the perforated disk. The great advantage of this form of apparatus for experimental work is the noiselessness of its rotation, admitting the close approach of the receiver without interfering with the audibility of the effect heard from the latter; for it will be understood that musical tones are emitted from the receiver when no sound is made at the transmitter. A silent motion thus produces a sound. In this way musical tones have been heard even from the light of a candle. When distant effects are sought another apparatus is used. By placing an opaque screen near the rotating disk the beam can be entirely cut off by a slight motion of the hand, and musical signals, like the dots and dashes of the Morse telegraph code, can thus be produced at the distant receiving station.

We have made experiments, with the object of ascertaining the nature of the rays that affect selenium. For this purpose we have placed in the path of an intermittent beam various absorbing substances. Professor Cross has been kind enough to give me his assistance in conducting these experiments. When a solution of alum, or bisulphide of carbon, is employed the loudness of the sound produced by the intermittent beam is very slightly diminished; but a solution of iodine in bisulphide of carbon cuts off most, but not all, of the audible effect. Even an apparently opaque sheet of hard rubber does not entirely do this. When the sheet of hard rubber was held near the disk interrupter the rotation of the disk interrupted what was then an invisible beam, which passed over a space of about twelve feet before it reached the lens which finally concentrated it upon the selenium cell. A faint but perfectly perceptible musical tone was heard from the telephone connected with the selenium. This could be interrupted at will by placing the hand in the path of the invisible beam. It would be premature, without further experiments, to speculate too much concerning the nature of these invisible rays; but it is difficult to believe that they can be bent rays, as the effect is produced through two sheets of hard rubber containing between them a saturated solution of alum. Although effects are produced as above shown by forms of radiant energy which are invisible, we have named the apparatus for the production and reproduction of sound in this way "*The Photophone*," because an ordinary beam of light contains the rays which are operative.

It is a well-known fact that the molecular disturbance produced in a mass of iron by the magnetizing influence of an intermittent electrical current can be observed as sound by placing the ear in close contact with the iron. It occurred to us that the molecular disturbance produced in crystalline selenium by the action of an intermittent beam of light should be audible in a similar manner without the aid of a telephone or battery. Many experiments were made to verify this theory without definite results. The anomalous behavior of the hard rubber screen suggested the thought of listening to it also. This experiment was tried with extraordinary success. I held the sheet in close contact with my ear, while a beam of intermittent light was focussed upon it by a lens. A distinct musical note was immediately heard. We found the effect intensified by arranging the sheet of hard rubber as a diaphragm, and listening through a hearing-tube. We then tried crystalline selenium in the form of a thin disk, and obtained a similar but less intense effect. The other substances which I enumerated at the beginning of my address were now successively tried in the form of thin disks, and sounds were obtained from all but carbon and thin glass. We found hard rubber to produce a louder sound than any other substance we tried, excepting antimony, and paper and mica to produce the weakest sound. *On the whole, we feel warranted in announcing as our conclusion that souuds can be produced by the action of a variable light from substances of all kinds, when in the form of thin diaphragms.* We have heard from interrupted sunlight very perceptible musical tunes through tubes of ordinary vulcanized rubber, of brass and of wood. These were all the materials at hand in tubular form, and we have had no opportunity since of extending the observations to other substances.

I am extremely glad that I have the opportunity of making the first publication of these researches before a scientific society, for it is from scientific men that my work of the last six years has received its earliest and kindest recognition. I gratefully remember the encouragement which I received from the late Professor Henry at a time when the speaking telephone existed only in theory. Indeed, it is greatly due to the stimulus of his appreciation that the telephone became an accomplished fact. I cannot state too highly also the advantage I received in preliminary experiments on sound vibrations in this building from Professor Cross, and near here from my valued friend Dr. Clarence J. Blake. When the public were incredulous of the possibility of electrical speech, the American Academy of Arts and Sciences, the Philosophical Society of Washington and the Essex Institute of Salem recognized the reality of the results and honored me by their congratulations. The public interest, I think, was first awakened by the judgment of the very eminent scientific men before whom the telephone was exhibited in Philadelphia, and by the address of Sir William Thomson before the British Association for the Advancement of Science.

At a later period, when even practical telegraphers considered the telephone as a mere scientific toy, Professor John Pierce, Professor Eli W. Blake, Dr. Channing, Mr. Clarke and Mr. Jones, of Providence, R. I., devoted themselves to a series of experiments for the purpose of assisting me in making the telephone of practical utility; and they communicated to me, from time to time, the result of their experiments with a kindness and generosity I can never forget. It is not only pleasant to remember these things and to speak of them, but it is a duty to repeat them, as they give a practical reputation to the often repeated stories of the blindness of scientific men to unaccredited novelties, and of their jealousy of unknown inventors who dare to enter the charmed circle of science. I trust that the scientific favor which was so readily accorded to the telephone may be extended by you to this new claimant—the photophone.

PLAN OF THE CEREBRO-SPINAL NERVOUS SYSTEM.

By S. V. Clevenger, M. D.

(Abstract from the paper (B 41) read before the American Association for Advancement of Science, Boston, August 28th, 1880).

The great French and German cerebral anatomists Luys and Meynert had endeavored to declare the architecture of the human brain from a multitude of microscopic sections, but so intricate were the relationship of fibres, nerve-cells, arteries, veins, connective tissue, etc., that it was at once seen to be necessary to study lower animal life anatomically and physiologically before the plan could be determined. Luys did nothing in this direction, while Meynert went as far as the brains of small mammals. Spitzka has carried the scrutiny still farther. The scheme of Meynert started with the upper part of the cerebrum as the seat of consciousness and, working downward, his "projection systems" ended in the periphery.

The nerve fibres composing the cerebrum and cerebellum were mainly considered. The presence of a multitude of nerve-cells and ganglia dispersed throughout this region was unaccounted for, and as these were of undoubted importance and all well known to anatomists, it was seen by pathologists that these schemes were insufficient.

No scheme can be correct which ignores any part of the nervous organization, or excludes any form of life as anomalous. The conclusion I have reached, is that the sympathetic system of vertebrata corresponds to the general nervous distribution of invertebrata above protozoa, presiding over the nutritive functions. The vaso-motor has been differentiated from the sympathetic distribution, whose office is to produce the vermicular motions of the intestines. Differentiation proceeds dorsally because that portion of the animal which is in most constant contact with the changing molecular motion of the environment would be precisely the portion to give origin to the higher series of nerve divisions. The endoderm, after the gastrula, stage remains under control of the sympathetic system. The so-called cerebral ganglia of Vermes, are homologous with the spinal segments which afterwards become coalesced in the vertebrata. This is the second system to be developed and *Amphioxus* has not acquired the third or cerebral system proper. In *Trigla Adriatica*, the third system series may be seen developed dorsally upon the second or spinal cord. This third system is the intervertebral ganglia. *Fusion of several of the higher intervertebral ganglia produces the cerebellum*, and accounts for the co-ordinating function of that organ. The several cerebral lobes, the tubercula quadrigemina, mammillary eminence, Gasserian ganglion, olfactory lobe, olivary body, etc.. are hypertrophied or atrophied (as the case may be) intervertebral ganglia. Projection systems and commissures, as the callosal, make their appearance in exact accordance with laws operative in the lower series.

The three systems develop gradually, and it may be said, commissurally one upon the other, and this scheme appears to account satisfactorily for physiological and pathological phenomena.

In addition to its publication in the proceedings of the Association, the paper will be produced in full, in the *American Journal of Nervous aud Mental Disease*, for October, 1880.

ANCIENT AGRICULTURAL IMPLEMENTS OF STONE.

By Hon. William McAdams, of Otterville, Ills.

In the rich, alluvial soil about the mouths of the Missouri and Illinois rivers are found many of these ancient stone implements used by the Mound-builders in their rude agriculture. Mr. McAdams exhibited a fine collection of these implements.

They are all chipped from flint, or a hard silicious limestone, and some of them beautifully made. Some are nearly a foot in length and six inches wide at the broader end.

Some are made to be fastened to handles, like our modern spades. Others resemble our modern hoes, having a deep, lateral notch, to facilitate the fastening to a handle. Some of these stone hoes are made with such ingenuity as to have been effective implements.

Mr. McAdams also exhibited stone implements which evidently were made to fasten to some kind of stock to be pulled through the ground like a plow. As these ancient people had no domestic animals for this purpose, it is probable that manual force was used to perform the work. The broad cutting edge of these stone implements was highly polished from long use by the attrition of the soil.

Mr. McAdams had found these implements of agriculture in the ancient graves associated with pottery, some of which contained carbonized corn. Cobs in a carbonized state were found, and the speaker is of the opinion that these ancient people lived principally on corn and vegetables, which they cultivated to a considerable extent.

The paper elicited much interest in the association.

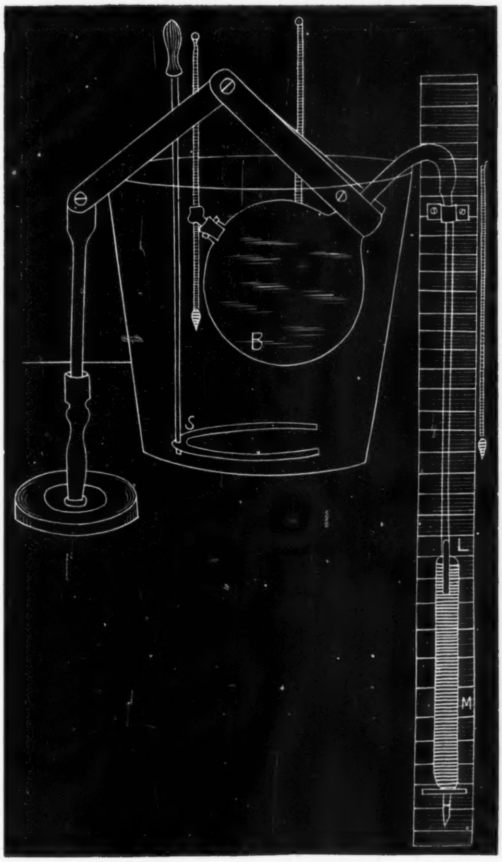
A very interesting report addressed to the committee of public health in France by M. Wurz, describes a process for retaining the green coloring of vegetables which is generally destroyed by boiling. It consists in the use of an excess of chlorphyl obtained from spinach (spinacia oleracea) which holds in its cells a large amount of coloring matter. A watery solution of this rendered alkaline by soda, is added to the boiling vegetable which is slightly acidulated with hydrochloric acid. The chemical result is common salt and a deposit of coloring matter on the organic tissue. There cannot now be any possible temptation for the unwarrantable dyeing of preserved vegetables by salts of copper or the employment of adulterants for obtaining that vivid coloring so attractive to the epicure.

ON THE CO-EFFICIENT OF EXPANSION OF GAS SOLUTIONS.

E. L. NICHOLS AND A. W. WHEELER.

The dilatometers used in this research differ essentially from those generally employed in the measurement of the expansion of liquids. Their form is shown in the accompanying figure. The bulb (B) contained about 286 grms. of the liquid—[at 15°C = 286.3425 grms.]

The dilatometer having been filled was placed in the bath in the position denoted in the figure. The lower end (*l*) of the vertical part of the neck was then immersed in mercury by sliding up the adjustable mercury tube (*m*) to the proper point. Upon cooling the liquid, the mercury rose in the neck of the dilatometer and from its height at various temperatures the volume of the solution was calculated.



This method was found to be of very great sensitiveness but its accuracy depends upon the careful observance of the following precautions. The neck must be accurately calibrated, the pressure must be kept nearly constant by re-adjusting, from time to time, the mercury-tube (*m*). Especial study must be made of the time which will elapse after each change of temperature; before the contents of the dilatometer will have assumed the same temperature as the bath. The authors found this interval of time to be 30 minutes for 1° and 2° intervals of temperature and 35 minutes for a 5° change. The bath itself must be constantly stirred. Finally the thermometers studied and not only their freezing points and boiling points ascertained, but a careful comparison must be made with some standard thermometer which in turn has been properly calibrated and compared directly with the air thermometer.

The following solutions of ammonia gas were used:

NUMBER.	Percentage of NH_3.	Specific Gravity at 14° C.
1	29.00	.9009
2	16.19	.9373
3	7.96	.9673
4	5.61	.9766
5	2.12	.9913

Of these the volumes at various temperatures from + 20 °C to the freezing points of the solutions or in the case of the stronger solutions to — 20°C were determined and the point of maximum density and freezing points noted. In the following table the volumes of each solution are compared with a unit volume of the same solution at + 4° C.

TABLE.

Volumes (observed values) of Aqueous Solution of NH_3 Gas.

(WATER)* PER CENT. NH_3=0.00.		PER CENT. NH_3=2.12.		PER CENT. NH_3=5.61.	
Temps.	Volumes.	Temps.	Volumes.	Temps.	Volumes.
20°.0	1.001744	14°.75	1.001114	19°.80	1.003180
18°.0	1.001348	9°.72	1.000413	14.80	1.001953
16°.0	1.000999	4°.76	1.000033	8.80	1.000726
14°.0	1.000701	4°.00	1.000000	4°.80	1.000106
12°.0	1.000451	2°.80	0.999958	4°.00	1.000000
10°.0	1.000345	1°.80	0.999941	1°.16	0.999594
8°.0	1.000114	— 0°.20	0.999941	— 1°.20	0.999398
6°.0	1.000030	— 5°.20	1.000296	— 4°.2	0.999298
4°.0	1.000000	--------	--------	— 5°.2	0.999272
2°.0	1.000073	--------	--------	—10°.2	0.999289
0°.0	1.000092	--------	--------	--------	--------

* The water volumes are taken from Rosetti's table.—Wüllner Phyrik Bd. III.

PER CENT. NH_3—7.96.		PER CENT. NH_3=16.19.		PER CENT. NH_3=29.00.	
Temps.	Volumes.	Temps.	Volumes.	Temps.	Volumes.
15°.00	1.002662	21°.80	1.007129	15°.00	1.007214
9°.80	1.001191	16°.80	1.004930	13°.01	1.005835
4°.80	1.000141	11°.80	1.002874	11°.06	1.004547
4°.00	1.000000	6°.80	1.001013	9°.15	1.003303
— 0°.20	0.999417	4°.00	1.000000	7°.28	1.002064
— 5°.20	0.998932	1°.80	0.999110	5°.33	1.000862
—10°.20	0.998740	— 3°.20	0.997514	4°.00	1.000100
—11°.20	0.998733	— 8°.20	0.996095	3°.27	0.999618
—12°.20	0.998760	—13°.20	0.994890	1°.16	0.998350
—13°.20	0.998794	—17°.20	0.994139	— 0°.80	0.997169
--------	--------	--------	--------	— 2°.51	0.996020
--------	--------	--------	--------	— 4°.38	0.994838
--------	--------	--------	--------	— 6°.40	0.993585

From these observed volumes the co-efficients of expansion were calculated and curves showing the volumes and co-efficients were plotted. The co-efficient curves are valuable in the determination of the points of maximum density, since they cut the base line at that point at a considerable angle, and serve to fix the temperature within 0.1° C.

The solutions of 2.12, 5.61, and 7.96 per cent. strength froze within the temperature interval reached by the common salt and ice freezing mixture used: the stronger solutions however remained in the liquid state. The following table gives the points of maximum density and the freezing points of the solutions:

Percentage.	Max. Density.	Freezing.	Saturation Pt.
0.00	4°.00	0°.00	100°.00
2.12	0°.80	— 5°.40(?)	93°.2
5.61	— 7°.20	—10°.6	83°.1
7.96	—10°.50	—14°.1	76°.4
16.19	-------	------	59°.0
29.00	-------	------	39°.8

It will be seen from these tables that the effect of ammonia gas in solution upon the water absorbing it is to increase greatly the co-efficient of expansion and to lower very rapidly both the points of maximum density and of freezing.

In these respects the gas acts just as a salt in solution would do. Gas solution and salt solution would seem to be closely related phenomena, each resulting in the formation of a mixed liquid, viz: of a liquid composed of two sets of independently moving molecules.

The effect of ammonia gas upon the volume of the water absorbing it is expressed by the following law:

When it is absorbed by water, the increase in volume for a constant temperature is directly proportional to the amount of gas absorbed.

This may be shown to be for NH_3 gas in water by plotting a set of curves with the volumes given in the above tables as ordinates and percentages of gas as abcissae. These curves, whatever temperatures be chosen, resolve themselves into straight lines. Since for the case of CO_2 gas in water the same law had been already found true by direct measurement of the change of volume due to the absorption of the gas at constant temperatures, we are warranted in suspecting the law to be a general one.

THE ENDOCRANIUM AND MAXILLARY SUSPENSORIUM OF THE BEE.

PROF. GEORGE MACLOSKIE, OF PRINCETON, N. J.

The endocranium of insects is produced by infoldings of the cranial wall, and although several groups (as Diptera, Hemiptera, Coleoptera, Lepidoptera,) have been represented as devoid of such structures, Prof. Macloskie finds an endocranium present in all these orders. The posterior or epicranial part of the skull has no internal processes. The clypeus, or "face," has a thick posterior ridge (just in advance of the antennæ). From this ridge descend, in bees and allied insects, two meso-cephalic pillars, reaching to the floor of the cranium, in front of the great foramen. These two pillars support the roof of the skull. They occur, with variations, in squash-bug, gadfly, mosquito, butterfly, and dragonfly. In the cockroach they take the form of a perforated plate, being united anteriorly by a cross-bar (which binds the mandibles together), and being webbed excepting at the centre. (Huxley's description of this in his Anatomy of the Invertebrates is inaccurate.)

The maxillæ and labium of the bee are supported by a long framework with elbows and hinges. This suspensorium is incorrectly represented in published figures. It is, in part correctly figured by Wolff, who misinterprets it (as if it were on the type of the mammalian skull). Its basal or posterior rods are attached close to the great foramen and to the base of the meso-cephalic pillars, and they are united by a thick web to the base of the skull. The mid-segment, consisting of a pair of bars, supports the maxillæ, and upon it is an anterior pair of bars supporting the labium. In its working, this frame-work embodies the principle of a recent patent for producing steady motion.

The methods by which the maxillæ and labium are protruded and withdrawn were described, also the relations and mode of working of the pharynx-parts in the mouth. The discovery of a double set of salivary glands was reported; a cephalic set supplying the inner tongue on the floor of the mouth, and the thoracic glands, sending their long duct forward to the labium. The inner structure of the bee's head was shown to be of the same pattern as in other insects, though varied in details. The paper was illustrated by diagrams and microscopic preparations.

NEW PLANETARY NEBULÆ.

BY PROFESSOR PICKERING.

He described the observations of the planetary nebulæ, are now in progress at the Harvard College Observatory. Besides measuring the light of these bodies, the spectrum of each has been examined by inserting a prism between the objective and eyepiece of the large telescope. A star is converted into a colored line of light, but the nebulæ, being nearly monochromatic, appears as a bright point. The difference is so marked that the idea suggested itself that by this means planetary nebulæ might be discovered, whose disks are so small that they can not otherwise be distinguished from stars. A search was accordingly undertaken on the evening of July 13th, by sweeping or moving the telescope so that a great number of stars could be examined in a short time. In a few minutes such a nebula was found, which with an ordinary eye-piece might readily be mistaken for a twelfth magnitude star. A similar object was also detected on the next evening. After this, sweeps on several evenings failed to reveal any new nebulæ, although it is estimated that the spectra of over a hundred thousand stars were examined.

On night before last, while continuing this work, an object with a remarkable spectrum entered the field. The light appeared to consist mainly of a band in the green, a line in the red and probably a fainter band in the yellow, the whole being superposed on a faint continuous spectrum. The new stars which blazed out in Corona in 1863 and in Cygnus in 1876, presented for a short time a similar spectrum, but with this exception the star noted above appears to be unique. It is too soon to form a theory regarding the nature of this body, as clouds interrupted the observations and barely allowed time for its identification. It proved to be the star known as Oeltzen 17681, and must therefore have had nearly its present brightness forty years ago.

The field for discovery by the method here given is far from being exhausted since, less than one hundreth part of the heavens has as yet been examined.

ON LAND SNAILS OF THE PALÆOZOIC PERIOD.

By DR. DAWSON, F. R. S., Principal of McGill University, Montreal.

The land snails occurring in the carboniferous and Devonian systems, of which six species are known, were noticed in detail. Two of these, *Pupa Bigsbii* from the coal formation of Nova Scotia, and *Strophites grandæva* from the Erian (Devonian) of St. Johns, New Brunswick, were described for the first time. Four of the known species belong to the different subdivisions of the old genus *Pupa*, and two are helicord or snail-like in form. They constitute a very isolated group of fossils, as none are known in older formations, and there are none newer till we reach the early Tertiary. Though all of somewhat distinct types, they all belong to one great family or sub-order of the *Pulmonifera*, and are all closely allied to types still living. All the species hitherto found are American, four being found in Nova Scotia and New Brunswick, and two in Illinois. The latter were discovered and described by the late Mr. Bradley. *Pupa vetusta*, the earliest known, was found in the material filling a hollow *Sigillaria*, by Sir Charles Lyell and Dr. Dawson in 1851. In the paper, which will probably appear in full in the AMERICAN JOURNAL OF SCIENCE, figures and descriptions of all the species are given, and their affinities and mode of occurrence are discussed.

FURTHER NOTES ON THE POLLINATION OF YUCCA AND ON PRONUBA AND PRODOXUS.

BY C. V. RILEY.

The author refers to the original paper on the Fructification of Yucca read at the Dubuque (1872) meeting of the Association and notices various criticisms since made upon its conclusions. The paper shows that none of these criticisms were warranted, and verifies the original observations and conclusions by subsequent experience. It points out the causes of error in that other writers have confounded related moths having similar *general* appearance but great structural differences and different habits. The characters of the Bogus Yucca Moth (*Prodoxus decipiens*), are given, and five new

species, *Pronuba maculata*, *Prodoxus marginatus*, *P. cinerius*, *P. æneseens* and *P. intermedius*), are described, and the paper concludes with remarks which point to these diffent Yucca Moths as admirable illustrations of the derivative origin of species.

THE WYANDOTTES.

By Major J. W. Powell.

The Indians now known as the Wyandottes, were first found on the lower St. Lawrence. Subsequently they inhabited a narrow district of country on the shores of Lake Huron, and were known as the Hurons; later they lived in Michigan about Detroit; then in Ohio in what is known as Wyandotte county; from Ohio they were moved to Kansas and placed on a reservation; and from Kansas to the Indian Territory. In their wanderings from point to point, as they were driven from advancing civilization, a few of their number were left behind, so that the Wyandottes are scattered from the lower St. Lawrence to the Indian Territory along the route of their migration. These Indians call themselves Wundat; the etymology of the word is not known. In their social organization four units are recognized—the family, the gens, the phratry and the tribe. The family, as the term is here used, is nearly synonymous with household. It is composed of the persons who occupy one lodge, or, in their permanent wigwams, one section of a communal dwelling. The head of the family is a woman. The gens is an organized body of consanguineal kindred in the female line. "The woman carries the gens," is the formulated statement by which a Wyandotte expresses the idea that descent is in the female line. Each gens has the name of some animal—the form of such animal being its tutelar god. Up to the time when the tribe left Ohio, eleven gentes were recognized as follows: Deer, Bear, Highland Turtle (striped), Highland Turtle (black), Mud Turtle, Smooth large Turtle, Hawk, Beaver, Wolf, Sea Snake, Porcupine. In speaking of an individual he is said to be a Wolf, a Bear, or Deer, as the case may be, meaning thereby that he belongs to that gens; but in speaking of the body of people comprising a gens they are said to be relatives of the Wolf, the Bear, or the Deer, as the case may be.

There are four phratries in the tribe—the three gentes, Bear, Deer and Striped Turtle constituting the first; the Highland Turtle, Black Turtle and Smooth Large Turtle the second; the Hawk, Beaver and Wolf the third; and the Sea-snake and Porcupine the fourth. The eleven gentes as four phratries constitute the tribe.

The civil government inheres in a system of councils and chiefs. In each gens there is a council composed of four women. These four women councilors select a chief of the gens from its male members; that is, from their brothers and sons. This gentile chief is the head of the gentile council. The council of the tribe is composed of the aggregated gentile councils. The tribal council, therefore, is composed one-fifth of men and four-fifths of women.

The government of the Wyandottes, with the social organization upon which it is based, affords a typical example of tribal government throughout North America. Within that area there are several hundred distinct governments. In so great a number there is great variety, and in this variety we find different degrees of organization, the degree of organization being determined by the differentiation of the functions of government and the correlative specialization of organic elements.

A SIMPLE DEVICE FOR PROJECTING THE VIBRATIONS OF LIQUID FILMS WITHOUT A LENS.

By H. S. Carhart, A. M., Professor of Physics and Chemistry, Northwestern University, Evanston, Ill.

This instrument is designed to project upon the screen the vibrations of a film of soapy water produced by the voice or by an organ pipe. It might be called the self-projecting phoneidoscope. It differs from Sedley Taylor's phoneidoscope in three particulars: first, the vibrations are communicated to the film through the agency of a mouthpiece and a ferrotype diaphragm; second, the vibrations are projected on a screen; third, the film is employed to project itself without a lens.

It consists of a wooden tube, having a telephone mouthpiece at one end and expanding into a large funnel at the other, the funnel being of metal. In the side of the tube a stop-cock is inserted. A film is obtained in the open end of the funnel and a little air is then blown through the stop-cock. This distends the film slightly, causing it to act as a convex mirror. It is then placed in a beam of *sunlight* and reflects it at the proper angle. Upon singing a note at the mouthpiece a sharply defined system of waves is projected. Photographs of these have been taken. Caps fitting into the funnel and provided with a square or triangular opening, are also employed to give films of different shape.

THE LANGUAGES OF THE IROQUOIS.

By Mrs. E. A. Smith.

The language of each nation represents its thought. If these thoughts have remained unrecorded, it is from the language itself that they must be obtained by tracing out the origin, history and meaning of its words. Each word has its history, which it can be made to reveal by tracing out the origin, history and their most hidden secrets, and the thoughts, customs and beliefs of the originator be read as truthfully as if recorded by the historian's pen. For "words unaided cannot lie;" twenty words in Tuscarora represent supernatural beings. Does this leave a doubt as to the tendency of their minds? The Tuscarora word for burial ground signifies "placed in the ground in a sitting posture," proving that some time in the past such was their method of burial. The very structure of the Indian languages, where the words are so self-explaining, affords unlimited scope to the etymologist in his search into word history. There are two distinct periods in the modern history of the Iroquois. The inundation of new ideas on the advent of the white man introduced almost a new vocabulary, differing according to the ideas of the observers. For instance, the horse when first seen by the Senecas was drawing logs, hence was called a log drawer. Another tribe saw it carrying packs, and termed it pack-carrier. The Tuscaroras adopted the English word and term it *ha-hath*. It is quite remarkable that so few words have been borrowed from the English. And these have become so Indianized by prefixes and appendages or changes in their vowel sounds as to be scarely recognizable. Among them are: U-ts—oats; Sa-i tar—cider; Ha-hass—horse; Vi-ni-gair—vinegar; Qui-tair—Peter; Ta-wait—David; Tju-rus—Julius; Nay-yak-it-ando—jacket. Lastly was-tun for Boston, adding to this the plural suffix ha-kah, a term which in English might be interpreted *ites*. We have then Was-tun-ha-kah, or Bostonites, which in the Iroquois is the general term for Americans or the whole American nation. This almost supernatural intuition of the Indian mind crystallizes, I do not doubt, the opinion also and belief of at least 250,000 pale faces residing in the metropolis of Massachusetts. Of the length of some of incorporative words, which sometimes contain verb, subject, object, adjective or preposition, I would remark that the examples generally given in encyclopedias and works on language are almost entirely English Indian. That is, a missionary, perhaps, translating a portion of the Bible, finds some abstract word entirely beyond the comprehension of the Indian mind; he therefore takes Webster's definition of the word and translates that into the Indian in the form of one word until it has the appearance of the heading to a German railway time-table, the words consisting sometimes of forty letters and eleven or twelve syllables. The longest word thus Anglo-Indianized with which I have met is the Mohawk word for stove polish, the word itself being as indicative of the ingenuity of the inventor as the polish itself. It consisted of a glowing description of all the excellencies of said stove polish, which it required fifty-eight letters to express. The abstract nouns, represented as being absent from many of the Indian languages, are found in the Tuscarora, such as life, death, love, hate. An interesting feature of the language also might be traced in the prefer-

ence given to the feminine gender instead of, as in the more ungallant English, to the masculine; for instance, the word theirs translates "two hers." The work I present is necessarily but a chrestomalthy compared to what can be done in the study of each of the Iroquois languages. Enough beauties, however, have been discovered through this mere insight to convince one that their possibilities were great. The reflection is, therefore, sad that in all probability fifty years hence these chrestomalthies, imperfect as they are, may be the only record of their former existence. Even now English is fast becoming the communicating medium of the people, as it is of the pulpit and the school. We can, therefore, safely predict that within the next century the Iroquois languages, as spoken by its six different tribes, will have become a thing of the past.

STRUCTURE OF MICA VEINS IN NORTH CAROLINA.

By W. C. Kerr.

At Danville, Va., Professor Kerr, of Raleigh, found veins or dykes which seemed to have been filled neither by fused matter nor by the ordinary mode of infiltration, but by a fine granular fragmented mass, derived from the containing bedded rocks, by the crowding, jamming and mechanical comminution of the rocks themselves. The mica veins in North Carolina are simply dykes of very coarse granite. When the crystallization becomes so coarse that the diameter of the mica sheets passes three or four inches, the dyke is called a mica vein. These veins are found in the upper Laurentian or Montalban, and may be considered characteristic of that horizon in North Carolina. The most productive veins are found in the high plateau between the Blue Ridge and the Smoky mountains, mostly in two or three counties. The amount of marketable mica produced per month is not more than two or three tons, although a much larger quantity could be obtained if the market demanded it. The most valuable of the present mica mines were opened and wrought by the mound-builders many ages ago on a much larger scale than now. There are evidences in the great river valleys in North Carolina of extensive glaciation in remote times, although the last glacial period is wholly unrepresented on the present surface. The protrusion of the eastern coast of North Carolina, about a hundred miles beyond the general Atlantic coast, is due to the interaction of the Arctic shore current and the Gulf stream, which collect the detritus thrown into the sea from Maryland to South Carolina, and drop them about Hatteras. This action has carried the coast of North Carolina to within fifty miles of the margin of the deep Atlantic channel, and, therefore, near its limit. The sounds behind the chain of sand islands or dunes, known as "The Banks," are rapidly silted up and converted into marsh and dry land by the sands blown over the dunes, and by the sediment brought down by the numerous rivers from the interior. The movement of the sand of these dunes was found to be about one foot per annum landward.

TRANSFORMATION OF PLANORBIS.

A PRACTICAL ILLUSTRATION OF THE EVOLUTION OF SPECIES.

By A. Hyatt.

The word evolution means the birth or derivation of one or more things or beings from others, through the action of natural laws. A child is evolved from its parents, a mineral from its constituents, a state of civilization from the conditions and surroundings of a preceding age. While evolution furnishes us with a valuable working hypothesis, science cannot forget that it is still on trial. The impatience of many when it is doubted or denied savors more of the dogmatism of belief than of the judicial earnestness of investigation. Every individual differs in certain superficial characters from the parent forms, but is still identical with hem in all its fundamental characteristics. This constantly recurring relationship among all creatures is the best established of all the laws of biology. It is the so-called law to heredity, that like tends to reproduce like. There seem to be only two causes which produce the variations which we observe; one is the law of heredity, the other is the surrounding influences or the sum of the physicial influences upon the organism. The first tends to preserve uniformity, the second modifies the action of the first. The law of natural selection asserts that some individuals are stronger or better fitted to compete with others, in the struggle of life, than are others of the same species: hence they will live and perpetuate their kind, while the others die out. An erroneous impression exists, that Darwinian doctrines are more or less supported by all naturalists who accept evolution, but it is far from the truth. The Darwinian hypothesis is so very easy of application, and saves so much trouble in the way of investigation, that it is very generally employed, without the preliminary caution of a rigid analysis of the facts, and it is safe to say that it is often misapplied. A great amount of nonsense has been written about its being a fundamental law, in all forgetfulness that we are yet to find a law for the origin of the variations upon which it acts; it cannot be the primary cause of the variations, for the laws of heredity are still more fundamental. The speaker then described the situation and character of Steinheim, where numerous shells of the Planorbidæ are found in the strata, which have been very regularly deposited. Hilgendorf claims to have discovered great evidences of the gradual evolution of the various forms from the simplest and oldest specimens, but Mr. Hyatt has failed to find what Hilgendorf describes. By means of a lantern a number of illustrations of the shells were projected upon a screen, and quite fully described. Four lines of descendants were shown to branch out from four of the simplest forms, with all the gaps between the species filled with intermediate varieties. Each one of the lines or series has its own set of characteristic differences, and its own peculiar history. It is a fair inference from the facts before us, that the species from the progressive series, which become larger and finer in every way, owe their increase in size to the favorable physical condition of the Steinheim basis. Darwinists would say that in the basin a battle had taken place, which only the favored ones survived. Mr. Hyatt endeavored to present, in a popular manner, the life-history of a single species, the *planorbis levis*, and its evolution into twenty or thirty distinguishable forms, most of which may properly be called by different names and considered as distinct species. He also endeavored to bring the conception that the variations which led to these different species were due to the action of the laws of heredity, modified by physical forces, especially by the force of gravitation, into a tangible form. There are many characteristics which are due solely to the action of the physical influences which surround them; they vary with every change of locality, but remain quite constant and uniform within each.

MOUNDS OF ILLINOIS.

By W. McAdams, Otterville, Ills.

Mr. McAdams stated that during a period of some 25 years, when leisure permitted, he had been exploring in the mounds of the State. Within a radius of 50 miles from the mouth of the Illinois river there were many thousands of mounds erected by the past inhabitants of the country.

A map was shown illustrating the ancient works of the region, which include almost every variety of mound in the Union. Mr. McAdams has explored hundreds of these mounds, and collected a great quantity of valuable material illustrating the habits and customs of the people of that age. He gave illustrations of House, Burial, Temple and other Mounds.

Many of the small mounds in this section, the speaker thought, were the remains of dwelling places, originally made by placing poles on end, or in a vertical position, fastened at the top, and the whole covered with sod and earth. This structure, after being repaired from year to year, would finally decay, fall to prices and form a mound. In many of these mounds he had found ashes, remains of animals eaten, and other articles that would be found in

such a primitive home. Of burial mounds there were several different kinas.

But comparatively few of the mounds contained valuable relics. In many of the mounds nothing at all of interest was found. It is an error to think that the ancient people always buried with the dead his personal effects.

He had, however, taken from mounds pipes, some of which are very peculiar, many kinds of sea shells, stone, copper and other ornaments, but seldom any weapons. Some of the copper ornaments shown were very curious and ingeniously made; among them were copper turtles, closely resembling the living animals, and large pipes of stone that represented the human figure in various positions. The speaker gave illustrations of mounds in which it would seem that sometimes on the death of their rulers a number of slaves or subjects were buried with him.

Mr. McAdams concludes from his explorations that the burial mounds show at least two distinct classes of people differing from our present indians.

The mound builders of the low lands of Illinois, like those of Ohio, were characterized by their peculiar pipes with the crescent base, the stem being a part of the base.

The potter makers, such as made the peculiar pottery of the region, were a different people, and imitated nature in their pottery, just as the mound builder did with his pipes. He had specimens on exhibition, and many illustrations showing this peculiar pottery representing men, animals, birds, fishes, shells and other things. The pottery makers' pipes were very unlike the mound builders', and were made for the insertion of a stem, the orifice generally being funnel shaped.

The speaker gave a spirited illustration of the great Temple mound, of Cohokia, opposite the mouth of the Missouri river, and describes it as a place of worship. This mound is 90 feet high. In the vicinity of this great mound were numerous flat square mounds called platforms. These platform mounds are usually ten or twelve feet high, and so large as often to contain on the summit farm-houses, with the out-buildings. In digging cellars, wells, etc., in these mounds, many relics were found; of these Mr. McAdams has a large collection. The speaker closed by describing a hitherto unknown earthwork, circular in form, one mile in circumference at the mouth of the Illinois river. Although the mounds occur in such great numbers and magnitude this seems to be the only earthwork in the region. Mr. McAdams expects to still prosecute his researches in this interesting locality.

DETERMINATION OF THE COMPARATIVE DIMENSIONS OF ULTIMATE MOLECULES; AND DEDUCTION OF THE SPECIFIC PROPERTIES OF SUBSTANCES.

By Prof. W. N. Norton.

In this paper a detailed exposition is given of the mechanical constitution of an ultimate molecule, the conditions of dynamical equilibrium are definitely stated, and several formulas investigated, representing its diverse mechanical features. From these definite mathematical expressions are deduced the general mechanical, physical, and chemical properties of substances. These are then employed in a detailed discussion of the properties of special substances. In this discussion the fundamental assumption is made that the atoms of different substances may differ in density, as well as in weight or mass. From this point of view it becomes possible to derive the comparative dimensions, and all the special features of the ultimate molecules of substances, from their molecular volumes and tenacities or co-officients of elasticity, as experimentally determined. The results of the numerical computations for a large variety of substances, from hydrogen to bismuth, are given in tables, and also represented graphically, and comparisons made with experimental results.

Chemical transformations are attributed to an effective force of electric tension developed by the contact of dissemilar molecules. An electro-motive force thus comes into play, determining an electric movement from one set of molecules to the other, and bringing them into approximate correspondence. The comparative values of the forces of electric tension, as well as of the electro-motive force, given in the tables, serve to make known the chemical relations of the substances considered. The chemical effects of heat are incidentally considered.

The entire discussion comprised in this and former papers may be epitomized as follows:

1—It has been shown that the mechanical laws and relations of bodies may be deduced from one general molecular formula; and that from their atomic weights, and certain comparative densities assigned to their atoms, may be derived definite expressions representative of the various properties of special substances.

2—We see that the deverse phenomena of Inanimate Nature are but different consequences of variations or inequalities of ethereal tension, produced by ethereal waves; and that, contemplated from the highest point of view, they may be conceived to result from the operation of one primary form of force on one primordial form of matter.

The publication of the papers read before the recent meeting of this Association will be continued in our next issue, September 18th.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

When a publishing house prints a date at the foot of a title page it is not always a guarantee to the public that the matter of the book has a connection with the date. In a play, a novel, or even a history, the date of a new edition only suggests that some class of readers desires another form of the work. Bnt when the subject of the publication is of such a character as to require additions in the progress of events, it is necessary to enlarge, remodel, or amend the contents, to suit the advance of knowledge and the public need. This is generally announced on the production of a new book. Its advertisement, if not made in the preface, is invariably embodied in a date appended to the title page. In fact, so general has this custom become, that I do not think any one, who takes up a new book of this kind for the first time, would neglect to cast his eye upon the date of publication.

The other day, looking over the well filled shelves of Messrs. Appleton & Co., I picked up a book of this progressive class, to whose pages I have turned with pleasure duiing many years, for amusement and instruction. Its concise statement of the advances in physical science had always struck me as most complete. I purchased the book (Arnot's Elements of Physics) for old aquaintance sake, and, on reaching my library, looked through its familiar pages for the latest discoveries; but imagine my disgust, to find that the edition of 1880 made no mention of Telephone, Motograph or Phonograph, three applications of science which will make the last decade one of the most brilliant of the century.

This may not be a commercial, but it is surely a *scientific* fraud.

D. O. Farrow.

What constitutes an artificial mineral water is an important question to the consumer, for obvious reasons, and to the importer it is a serious matter, as commercial rivalry and custom duties have forced its consideration upon them and the authorities. Trouble has been caused in other countries, also, for want of a proper definition, and it has given rise to a German imperial decree in which a solution of the difficulty is attempted. This decree, reads as follows: "Under artificial mineral waters are included not only imitations of certain mineral waters as they occur in nature, but also is understood such other artificially prepared solutions of mineral substances as represent mineral waters, without corresponding in their chemical composition to natural waters."

BOOKS RECEIVED.

A Physical Treatise on Electricity and Magnetism. By J. E. H. Gordon, B. A., Camb., Assistant Secretary of the British Association. In two volumes. [London : Sampson Low, Marston, Searle and Rivington. 1880.

One of our correspondents calls attention to what he considers a breach of privilege on the part of a publishing house, which affixes the date 1880 to a scientific textbook which does not mention the telephone or motograph. If this omission be a sin it is simply a sin of omission; but quite different is it in the case of Mr. J. E. H. Gordon, an English compiler of a work on electricity and magnetism, in two volumes, which has come to us, with a flourish of trumpets, across the Atlantic. This book, which gives many pages (we will not say, however, too many, as the subject is an interesting one) to De La Rue's beautiful experiments, and eighteen pages to Mr. Crook's etherial and radiant speculations with Mr. Gimingham's pretty tubes, only condescends to notice the Bell telephone in one brief page, and entirely ignores the existence of Edison's carbon telephone; although he recognizes the principle of the latter in Hughes' microphone, to which he gives great credit in another page. Aside from the unpardonable negligence evinced in this want of literary balance, which shows Mr. Gordon's incapacity as a book compiler, we here have a recurrence of an indignity unworthy of an Englishman. Mr. Gordon (who is an Assistant Secretary of the British Association) knows, or ought to know, that Mr. Preece exhibited an Edison musical telephone to the Association at their Plymouth meeting in 1877; and also that Edison's agent in London showed Mr. Hughes the carbon button and its properties in a telephone, three weeks before Mr. Hughes picked up the eliminated defects of the button as the principle of the microphone.

It does seem as if this ignoring the great services of the American Edison is but a part of a scientific conspiracy to falsify history. Where is the tasimeter, the most delicate electrical instrument for the measurement of radiant energy known to science? Where the motograph, that inexplicable wonder, which a telegraph company (more appreciative than Mr. Gordon) thought worth a hundred thousand dollars, the price they offered and paid for it? Where are their descriptions to be found in Mr. J. E. H. Gordon's Physical Treatise on Electricity and Magnetism? He does not deign to pen one line on the subject. This is either ignorance or folly; let Mr. Gordon accept which horn of the dilemma he thinks better.

However, considering the hasty manner in which the text of this book is thrown together, it can scarcely pass into currency, except as a beautifully illustrated catalogue of inventions and discoveries in which Mr. Gordon took no part. In this compilation (without reference, let it be distinctly understood, to the distinguished authors whose works are woven in without decent order or proportion), Mr. Edison shines by his absence.

The book cannot yet be purchased here, as the American buyers of the copyright are keeping it for the fall trade. We regret their connection with it, for what popularity can be expected, in this country, for a work on electricity that ignores the existence of Henry, Morse and Edison?

Manual of Hydraulic Mining for the use of the Practical Miner. By T. F. Van Wagenen, E. M., New York, D. Van Nostrand, 1880.

Of all the problems presented to the mining engineer, there is none more important, nor simpler, than that contained in the subject of hydraulic mining of gold. It is only necessary to be sure of the premises and the results may be considered certain. There are, really, but two questions involved, water to move the soil, and place to put it in. If these conditions are fulfilled, it is not difficult to predict success to those who have but fair promise of paying ground. Once we know where is the dumping ground, how high the fall, and what the grade at command for sluicing boxes, all that has to be done is to bring water to the highest point above the workings; which, of course, presupposes it has been lead from the source to the place of fall on the least grade consistent with a sure and economical supply.

Much of the brain and sinew of the working classes in the far West has taken to this class of mining, as offering the most enduring profit and employment; but hydraulic mining requires something else besides mere will and muscle. For its successful application a certain knowledge of figures, rather than of mineralogy, is requisite. These hardy men do not always possess such knowledge, and for their instruction, Mr. Van Wagenen has written a little manual which will be read, studied and understood by many a practical miner.

The book can serve as a model for writers who have something valuable to say, and who wish to speak to men who have no desire to waste time in hunting for the truth.

PHYSICAL NOTES.

The new electro-dynamic law of Clausius is receiving the deepest attention from the first electricians and mathematicians of Europe. The fundamental character of all his work and the acknowledged preëminence of his views, immediately demand an early investigation at the hands of his compeers. Already have Lorberg, Delsaulx, Frölich and others submitted this law to rigid analysis.

This new law of Clausius was advanced by the distinguished author only after finding himself unable to reconcile the two laws of Weber and Rieman with that simplicity which overwhelmingly addresses itself to our reason; and because they seem too complicated to be used as explanatory of these molecular currents so felicitously employed by Ampère in his theory of magnetism. These laws of Rieman and Weber require us to believe in the existence of two equal and opposite currents as originating all electrical action. In another essential point, Clausius (Annalen der Physic und Chemic X., 4, p. 609), finds himself compelled to differ from Rieman and Weber. They assume a *relative* motion between the electrified particles. Rieman using the word in its ordinary acceptation, according to which the difference of the components of velocity of the two electrical particles is made to represent the components of velocity of the relative motion; and Weber referring the relative motion to the mutual advance and retreat of the particles. Clausius rejects this method, which would confine one in the consideration of the subject to relative motion, and treats of both individual motions of the particles in their action on each other.

In the same number of the Annalen is an article by Herr Budde, on the laws of Clausius, in which, as an adherent of the theory, he exposes the fallacy of Frölich's interpretation.

The contrary effects of sunlight in relation to certain chemical compounds, is noted by T. P. Blunt (*Analyst.* 1880, 79–81), who finds that an oxalic acid solution exposed to the light is rapidly decomposed, which is not the case in the dark. If this observation of Mr. Blunt is substantiated the use of that valuable re-agent in stoichiometry, where it serves as a basis for standardizing, will have to be restricted. From the ease with which oxalic acid is dried and weighed, and its non-corrosive nature, it has been considered almost invaluable in the working laboratory.

Mr. Blunt, on the other hand, finds that ferrous iodide requires the light in order to prevent decomposition. Does not this anomalous action of light point to a mechanical association and dissociation of molecules, analagous to that separation of tangible bodies effected by sound, as seen in Chladni figures?